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Scientific problems as such are the concern of the various scientific societies and are examined in their publications, but these societies do not—except very occasionally for some special reason—inquire into the relation of the problems to other realms of study. Similarly, the problems of general philosophy are the concern of philosophical societies and are examined in their publications, but such consideration as they give to science is incidental and is undertaken because the examination of science is found to be necessary for the understanding of science. We do not, of course, disparage either of these undertakings; on the contrary, we regard them as essential to a complete education. They do however leave a conspicuous gap¹

To fill this gap our Group was formed seven years ago following a meeting convened by Professor Herbert Dingle—our founding father—on 8th March 1948, and it is now just over five years since the first number of our *Journal* appeared in May 1950. During this time 63 meetings of the Group have been held in London, a very active Northern Branch has been founded and our *Journal* has attained a world-wide reputation. Nevertheless, since 1948 there has been no formal discussion of the nature, scope, and purpose of our activities considered as a whole. The present flourishing state of the Group and of its *Journal* should not preclude us from engaging in such discussion on the grounds that it is unnecessary, for our critical activities ought to be directed just as much to our own discipline as to the special sciences.

In the Foreword to the first volume of our *Journal* from which I have quoted it is stated that 'in general, science, both past and present, will be taken as the primary datum for consideration and the consideration will be that of the philosophical implications contained in it'.

* Based on the Chairman's Address to the Philosophy of Science Group of the British Society for the History of Science, 10th October 1955.

¹ Herbert Dingle, this *Journal*, 1950, I, 3

Although we are both willing and anxious that professional philosophers should interest themselves in our activities, and indeed we have already received from many of them valuable help and encouragement, our Group was formed primarily for scientists. Despite the fact that many have responded, it would be idle to pretend that all wish us well. Since our appeal is directed mainly to them, perhaps it is not surprising that some are not merely indifferent to our activities but are definitely hostile. This opposition to our subject is one which is not rooted solely in philistinism and prejudice, and we ignore it at our peril. Therefore, before formulating our defence it is essential that we should attempt to understand and analyse the opposing point of view.

I *The Case against the Study of the Philosophy of Science*

(a) Perhaps the easiest point of criticism to dispose of is one of the commonest—that our subject attracts the intellectually ‘half-baked’! Only the most fanatical puritan objects to feminine beauty because it attracts the libertine, and it is equally fantastic to condemn the study of a subject because some of those who pursue it are cranks.

(b) A much more serious source of criticism concerns that peculiar modern fetish—time. Some heads of scientific departments have been known to declare that they will not tolerate members of their staffs spending time on our subject, either because time cannot be spared for, or because it ought not to be wasted on, those questions which come within our purview. Although part of this criticism can be written off as mere philistinism, there remains a residue which goes deeper. This residue has both a modern and a perennial aspect. The former is associated with the policy which may be described colloquially as ‘keeping one’s eye on the ball’; the latter with the traditional rivalry between scientists and philosophers, which is still conditioned by the belief of many men of science that all philosophers implicitly, if not explicitly, regard their subject as a kind of ‘governess science’ endowed with the right to supervise the special sciences and keep them in order.

The question ‘what is the *use* of the philosophy of science?’ and the related question ‘what *is* the philosophy of science?’ are often asked, and it may be assumed that most scientists opposed to our subject have failed to find satisfactory answers. Moreover, even when it is recognised that there is such a subject and that it is not totally unworthy of study, it is tempting to argue that the pace in science is now so hot that one simply cannot afford to be distracted from specific questions

to attend to wider issues. The young and brilliant are therefore being increasingly advised for the sake of their careers to concentrate their attention on narrow fronts.

The case thereby implied against the study of our subject, at least by active scientists, is a strong one and must be resolutely faced, but not like the knotty theological problem of which the preacher said 'having stared it boldly in the face, let us pass on!' This case is still further strengthened by the fact that other issues than purely personal ambition are involved. Recently, the view has been widely expressed that international rivalry is tending more and more to assume the form of technological competition. The foundations of modern technology, whether physical or biological, lie in the pure sciences, and in the long run it is the pure scientists who set the pace. Consequently, it can be argued that by 'keeping his eye on the ball' the scientist is not merely furthering his own career but also the essential interests of the nation to which he belongs.

At first sight it must be admitted that this is a most powerful argument against the study of our subject, at least at the present time. Those scientists who philosophise would seem to be like the characters in *The Cherry Orchard* of Chekhov who sit down to discuss God, Man, and the Universe while the estate is about to be sold over their heads.

(c) I have stated the case against the study of our subject by scientists, but it is often stated, and even more often implied and insinuated, that philosophers too ought not to waste their time upon it. For example, in the Introduction to an excellent account of the foundations of modern atomic theory one of our most distinguished theoretical physicists, having made the perfectly valid point that 'It was not the philosopher Kant but the mathematician Gauss who first discovered that the three angles of a triangle might not add up to two right angles', then went on to claim, a few sentences later, that 'There are too many alternatives offered in the undirected speculations of the philosophers'.¹ Now, not only are these two points mutually contradictory, but the latter is founded on a widespread misconception. Indeed, in so far as professional philosophers err in their conception of scientific theories it is usually in the opposite sense to that suggested here. Thus, the great defect of Kant's analysis of space was that it *excluded* alternatives and so placed fetters on the geometer's powers of imagination. As Philipp Frank has so incisively argued in his essay 'Why do scientists and

¹ C. G. Darwin, *The New Conceptions of Matter*, London, 1932, p. 7

philosophers so often disagree about the merits of a new theory?', philosophers tend to err, in their analysis of science, not so much by indulging in undirected speculation as by transforming tentative physical hypotheses into immutable philosophical principles and thereby petrifying them.¹ Kant's mistake was not that he failed to appreciate Newton's achievement but that he was, in physics, 'plus royaliste que le roi'. Kant transformed Newton's laws into allegedly self-evident principles which cannot be changed. Newton himself was more cautious, and in his *Queries* at the end of the *Opticks* he wrote that

it may be also allow'd that God is able to create Particles of Matter of several Sizes and Figures, and in several Proportions to Space, perhaps of different Densities and Forces, and thereby to vary the Laws of Nature and make Worlds of several sorts in several Parts of the Universe. At least, I see nothing of Contradiction in all this.²

Indeed, in so far as a valid case can be made against the study of the philosophy of science on its own ground and without reference to the issues discussed in Section (b) above, it applies *in the main* to philosophers who are not scientists rather than to those who have been trained in science, although it must be admitted that Kant himself was abreast of the science of his day. On the whole, philosophers of science who are not themselves trained in science tend to have an exaggerated respect for current theories. (Similarly, scientists who are philosophically naïve often tend to fall into the same error.) A striking case in point occurred recently. A well-known philosopher at Oxford in a review of a book on Freud wrote as follows :

He claimed a knowledge of unobservables and yet he knew with certainty the form that this knowledge must assume. The strange dream-logic of dichotomies and identification of opposites, of transference, ambivalence, displaced symbols is to be taken with entire literalness as an engineer's drawing of the actual workings of the mind at its deepest levels. There is no suggestion that Freudian theory is one amongst a set of possible theories, pictorially or metaphorically expressed. It is simply to be taken as fact.

Must one accept this claim? . . . the whole scheme of infantile sexuality and its development, of the typical family situations, of repression, sublimation, and transference, has been confirmed as the truth. . . .³

¹ P. Frank, *Modern Science and its Philosophy*, Cambridge (Mass.), 1949, p. 207

² I. Newton, *Opticks*, London, 1931, pp. 403-404

³ S. Hampshire, *The Observer*, 9th October, 1955

PHILOSOPHY OF SCIENCE

We can therefore sum up the case against the study of the philosophy of science on two main counts : (i) scientists who pay attention to the subject are wasting their time and diverting their energies from more important activities ; (ii) philosophers who study the subject are inclined to be too eager to accept the latest achievements of scientists as final and thereby tend to reduce science to a state of petrification.

2 *The Case for the Study of the Philosophy of Science*

(a) M Jourdain in Molière's *Le Bourgeois Gentilhomme* was astonished to discover that for more than forty years he had been talking prose. Similarly, it must come as a shock to many a hard headed scientist to be told that he has a philosophy of science—either a conscious or an unconscious one—for a scientist who deliberately neglects philosophy does not think in a mental vacuum. Instead, he tends to be, in Whitehead's phrase, 'the victim of the chance philosophic prejudices imbibed from a nurse or a schoolmaster or current modes of expression'. Philipp Frank has put the matter very forcibly :

One thing seems to be certain : if we try to eliminate from, say, physics all teaching of the philosophy of science, the result will not be a crop of scientifically minded physicists, but a flock of believers in some fashionable or obsolete chance philosophy.

Among students of science, the students of engineering are those who traditionally get the worst training in philosophic analysis. They often absorb science stripped of its logical structure, as a mere collection of useful recipes. Is it only a coincidence that the students of engineering have on the whole been more impressed by empty political slogans (like Fascism) than the students of 'pure' science ?¹ There is no doubt that general slogans play a rôle in politics similar to the rôle that general principles play in science. If someone is trained to understand to what degree general principles like conservation of energy or relativity are based on confirmable facts and how far on arbitrariness and imagination, he is more immune to the political slogans of demagogues than a student who has been trained only to record his immediate experience and to regard the general laws as gifts dropped from heaven for helping him to bring some order into his record sheet.²

A contemporary philosopher of science who based his philosophy of science solely on nineteenth-century science would be justly censured ; but how many scientists are there today who combine up-to-date

¹ Frank was, of course, writing with his Central European experience in mind.

² P. Frank, *op. cit.*, p. 266

knowledge of their particular field of study with a nineteenth-century philosophy of science as a whole? Far too many, and it should be the object of this Group to reduce their number.

(b) Philosophy of science is a controversial subject, but oddly enough its finest products have a quality of permanent interest which exceeds that of most other scientific writings. Similar claims have been made on behalf of other subjects. In his celebrated but much criticised *A Mathematician's Apology*, published in 1940, G. H. Hardy made great play with the permanence of mathematical achievement. He quoted what he then called 'some rather painfully rhetorical sentences' from his Inaugural Lecture delivered at Oxford in 1920, in which he said of the activities of pure mathematicians that 'what we do may be small, but it has a certain character of permanence; and to have produced anything of the slightest permanent interest, whether it be a copy of verses or a geometrical theorem is to have done something utterly beyond the powers of the vast majority of men'. In his *Apology* he claimed that Greek mathematics, for example, is 'more permanent than Greek literature. Archimedes will be remembered when Aeschylus is forgotten, because languages die and mathematical ideas do not'.¹ Against this sweeping claim may be placed a passage from an essay on 'The Value of Greece to the Future of the World', written in 1922 by Gilbert Murray.

The time has come for Euclid to be superseded: let him go. He has surely held the torch for mankind long enough; and books of science are born to be superseded. . . . But when we read Homer or Aeschylus, if once we have the power to admire and understand their writing, we do not for the most part have any feeling of having got beyond them.²

It is not our business to attempt to adjudicate between these rival claims, but instead to make a claim of our own which is beyond dispute, for we are entitled to point out that the classics of our subject have this great merit: they are not merely respected but are widely studied in the original (or in translation). Philosophers of science may, in the opinion of many, be men who bicker³ while others build, but of each

¹ G. H. Hardy, *A Mathematician's Apology*, Cambridge, 1940, p. 21

² G. Murray, *The Legacy of Greece* (ed. R. Livingstone), Oxford, 1922, p. 5

³ For a cogent discussion of the view that the advancement of learning can only be understood as 'a very human adventure of quarrelsome individuals' and that if the quarrellers are eliminated 'you get only further accumulation of knowledge in terms of the premises frozen into the philosophy of the state', see J. B. Conant, *The Citadel of Learning*, Yale, 1956.

of the greatest we can say that, as Hilaire Belloc hoped would be said of him when dead,

‘His sins were scarlet, but his books are read.’

In the last of three stimulating broadcast talks in March 1955 on ‘The British Universities’, Dr Eric Ashby, Vice-Chancellor and Principal of The Queen’s University, Belfast, concluded by lamenting that ‘contact with genius is not obligatory even for honours students in any university faculty except arts’. He illustrated this from his own subject—botany.

I believe it is possible [he said] to get a first class honours degree in botany at any British university without having read a work of genius. The student must be familiar with the gist of what some of the great biologists have thought, but for that you do not need to read what they wrote; it is easier to get it out of text-books written by competent hacks. What the student must do above all is to familiarise himself with the latest research monographs, still damp from the press, however pedestrian their authorship. But (except possibly in the essay paper, and then only by good luck) it would profit the botany student nothing to have read Darwin or Hofmeister or Linnaeus or Aristotle; and the physics student who read Newton or Rutherford would be wasting his time. How different it is for the music student, who must soak in Bach and Beethoven if he is to get a good degree; or the philosopher, who must soak in Plato and Descartes; or the classicist, who must soak in Sophocles and Thucydides and Virgil. [And he wondered] whether faculties of medicine and science and technology have not still to learn a simple and vital lesson from the long tradition of the teaching of arts subjects: namely to bring the student face to face with genius.

This criticism does not apply to the study of the philosophy of science. The greatest pure mathematician and one of the greatest mathematical physicists at the turn of the century was Henri Poincaré. At the present day for each occasion that someone somewhere consults one of his five hundred or more mathematical papers and books there must be at least a hundred occasions on which someone somewhere is reading one of his philosophy of science essays. I do not propose to argue whether his work as a philosopher of science was of greater or of lesser significance than his work as a mathematician. The point I wish to make is that his philosophical works are still widely read, whereas his mathematical writings are not, and the passage of time is likely to accentuate the difference. This is but one example.

Generally speaking, the classics of our subject are not merely admired in principle: they are read in practice.

I firmly believe, however, that knowledge of these classics ought to be still more widely diffused. Even if it were agreed that what Aristotle wrote about the structure of the heavens, the laws of motion, and the anatomy of animals were of interest only to the historian of science, and I for one would not accept this verdict without serious reservations, what he wrote about our subject ought still to be, at least in essence, familiar to the scientist and not only to the historian. The reason is that in his writings we find the first systematic enunciation of a point of view concerning the nature of science which has a major influence on subsequent thought on the subject—at first directly and later indirectly by the reaction against it. Again, although Newton's *Principia* ought to be, and is, still read as a whole by some scientists, it is much more important that his remarks which bear on our subject should be studied by as many as possible. *Hypotheses non fingo*: how often is this quoted or appealed to by scientists who have no idea of the context and intention? Or, consider Francis Bacon: although he contributed little to science itself, his contributions to the philosophy of science were important and are still frequently appealed to by scientists who have only a vague idea of what he said and an even hazier idea of the serious criticism to which his views have been subjected.

With these considerations in mind, it seems to me that there is a specific service which a journal such as ours could perform: from time to time a paper could be published on one of the great masters of our subject, presenting within the space of, say, five or six thousand words a concise summary and assessment of his philosophy of science, of its subsequent influence and its significance for us today. For example, one such paper might be devoted to Aristotle, another to Mach, another to Poincaré, another to T. H. Huxley, another to Duhem—and so on. I have deliberately chosen possible names at random, for such a suggestion if it were to be acted upon would almost certainly be somewhat random in its operation, depending on when suitable articles come to be written. It should not, however, be the intention to offer this series as a substitute for the study of the philosophers of science in their own writings, but on the contrary to stimulate still greater interest in these writings than is already evident. Indeed, such a series could (i) make more scientists aware of the actual achievements of those who have contributed significantly to the subject, (ii) draw

PHILOSOPHY OF SCIENCE

attention to the original sources, their most accessible and satisfactory editions and, where necessary, translations, and (iii) serve to emphasise the point that, strictly speaking, there is no philosophy of science but only *philosophies* of science of different points of view and different degrees of profundity.

(c) Philosophy of science has, of course, as much right as any other discipline to be studied for its own sake by those who wish to specialise in it. In justifying a more widespread study of the subject, however, I have already drawn attention to the fact that almost all scientists have their philosophies and that it is better to have a consciously thought out and, if I may say so, a 'scientific' philosophy of science than an unconscious, dogmatic, and 'unscientific' philosophy of science. Moreover, the subject has, I believe, a peculiar educational value which is particularly evident at meetings when specialists in widely different fields are brought together. For its study can be a powerful antidote to the inherent tendency in science itself towards ever increasing specialisation. It is true that cross-currents are continually being set up, and processes of cross-fertilisation occur between the various special sciences but generally these give rise to new border-line subjects which rapidly develop into new specialisations resulting in the further fragmentation of science. Only in study and discussions pertaining to our subject is it possible, as a rule, for serious consideration to be given to the nature and tendencies of science as a whole, and even of the special sciences considered both separately and in relation to one another.

(d) I now come to a more controversial point in the defence of our subject: is the study of the philosophy of science of any help to the scientist, not merely when he is talking about science but when he is actually doing science? In other words, can it be of any help to him professionally? As I have already indicated, the gravamen of the case against the study of our subject is to be found here: that, in fact, time spent on the study of our subject so far from being of any help to the working scientist is a positive hindrance to him.

Before attempting to rebut this charge, we must admit that just as science itself can be misapplied so can the philosophy of science. There is a time and place for everything, and a scientist who was puzzling over a problem of philosophy when he ought to be looking for a leak in his apparatus would be justly censured. Physics, however, is more than plumbing and scientists are not mere mechanics. Broadly speaking, the tangible outcome of any scientific investigation is either the creation of an instrument or the writing of a paper for a learned

society or journal. It is with the latter objective that we are now concerned. Only if one has had the misfortune to be called upon fairly frequently to referee papers submitted to scientific societies and journals is it possible to realise how badly composed most of them are! The object of writing a paper (other than to be able to list it in an application for a post or in an annual report) is *communication*. Unfortunately, not more than one scientific paper in five, if that, seems to be written with this object consciously in mind. It is not so much that the authors are illiterate as that they have seldom thought out how to convey in their language, grammatical or ungrammatical, the results of their experiments and calculations. Even when on first appearance a paper seems to be well presented, closer inspection often reveals that the author assumes that the reader will accept without question all manner of unstated assumptions and involved arguments presented in a truncated form as if their very brevity ought to be taken as sufficient guarantee of their validity. A more thorough training in logic and the philosophy of science should lead to a more critical attitude towards the manner of presentation of the results of scientific research.

Indeed, it is already gradually dawning on many of those whose duty it is to teach future scientists and technologists in an age when science and technology are tending to replace the humanities as the intellectual foundations of our civilisation, that the deliberate cultivation of critical standards is no less essential for the student of science than for the student of literature. It has usually been assumed that students need only be taught the facts and techniques of science and that they should be left to discover for themselves intuitively how to think and how to express themselves. The study of *the method and mode of presentation* of the classics of science has usually been neglected while attention has been concentrated solely on the results obtained and their place in the general body of knowledge: the finished product, rather than the process of manufacture, has been the object on which the student has been encouraged to focus his attention. Instead, I would suggest that the educational curriculum of the future scientific research worker should include the study of critical analyses, made by philosophers of science, of selected classics of scientific investigation.

I do not want to claim, however, that *all* philosophers are models of clarity. In his recent admirable essay *On Philosophical Style*, Professor Brand Blanshard quotes Clifford, himself a master of exposition, as remarking about an acquaintance:

PHILOSOPHY OF SCIENCE

He is writing a book on metaphysics, and is really cut out for it ; the clearness with which he thinks he understands things and his total inability to express what little he knows will make his fortune as a philosopher. Unhappily [says Brand Blanshard] the gibe has point. There are philosophers or pseudo-philosophers, to understand whom would be a reflection on the reader's own wit.¹

Moreover, even some great philosophers have failed to put their thought across to other men of great intelligence, partly because they have been grappling with difficult and elusive ideas and partly because they have tended to forget that most men's minds are so constructed that they have to think by means of examples rather than by sweeping generalisations. However, on the whole, with one or two outstanding exceptions, these strictures do not generally apply to the great philosophers of science, and so we can advocate, without serious reservation, the value of our subject for the training of the scientist.

(e) Finally, we come to the fundamental question of the relevance of our subject to scientific research itself. In other words, can the philosophy of science contribute significantly to the advance of science, i.e. to the actual process of scientific discovery ?

I believe that the basic reason why so many scientists do not feel that it is worth while spending time on the study of the philosophy of science is not because they reject the claim that the subject helps to sharpen the critical, particularly the self-critical, faculty, nor because they deny that it can assist the student to appreciate the scientific approach to a problem—although both of these claims are often ignored or rejected—but rather because they consider that philosophers have entertained grossly exaggerated notions of the subject which they call 'scientific method'. The search for this method as a master-key to unlock the secrets of nature seems to bear too strong a resemblance to the search for the perpetual motion machine and the philosopher's stone to be tolerated any longer. Typical of the attitude of many scientists in this respect is the sweeping assertion made by the author of a recent fascinating book with the significant title *The Art of Scientific Investigation* :

There is a vast literature dealing with the philosophy of science and the logic of scientific method. Whether one takes up this study depends upon one's personal inclinations, but generally speaking it will be of little help in doing research.²

¹ B. Blanshard, *On Philosophical Style*, Manchester, 1954, p. 28

² W. I. B. Beveridge, *The Art of Scientific Investigation*, London, 1950, p. 7

While sympathising with the view that in the past philosophers have tended to misconceive the nature of scientific method, we should not allow an extreme statement such as this to go unchallenged. Although no one can deny that scientific discoveries have, in fact, often been made by men who were innocent of any knowledge of philosophy and even of logic—indeed have sometimes been made by apparently flying in the face of logic, e.g. by Heaviside and by Dirac—there are important instances where scientific discovery has been the fruit of scientific philosophy. There is no doubt that in his work on the foundations of dynamics, Galileo was guided quite as much by his philosophy of science as by the famous experiments—some of which he probably never performed and others which he may well have ‘fudged’! As for Einstein, who would deny that his life’s work was primarily the product of his philosophy of science and that this philosophy was itself shaped by his study of Hume and Mach, Kant and Poincaré? Indeed he himself maintained that ‘The present difficulties of his science force the physicist to come to grips with philosophical problems to a greater degree than was the case in earlier generations’.¹

Although it seems to me that Professor Ayer tends to place too strong an emphasis on the purely empirical in deciding whether a scientific system is valid, and therefore argues that a philosopher, *qua* philosopher, cannot assess the value of any scientific theory, I fully agree with him in the following passage, except that I should like to substitute the term ‘philosopher of science’ where he says ‘philosopher’:

It might be thought that the philosophical elucidation of scientific theories was required only for the popularisation of science, and could not be of much benefit to the scientists themselves. One has only to consider the importance to contemporary physics of Einstein’s definition of simultaneity in order to realise how necessary it is for the experimental physicist to be furnished with clear and definitive analyses of the concepts which he employs. And the need for such analyses is even greater in the less advanced sciences. For example, the failure of psychologists at the present time to emancipate themselves from metaphysics, and to coordinate their enquiries is principally due to the use of symbols such as ‘intelligence’ or ‘empathy’ or ‘subconscious self’ which are not precisely defined. The theories of psycho-analysts are particularly full of metaphysical elements which a philosophical elucidation of their

¹ A. Einstein, *The Philosophy of Bertrand Russell* (ed. P. A. Schilpp), Chicago, 1944, p. 279

PHILOSOPHY OF SCIENCE

symbols would remove. It would be the philosopher's business to make clear what was the real empirical content of the propositions of psycho-analysts, and what was their logical relationship to the propositions of behaviourists or *Gestalt* psychologists, a relationship at present obscured by unanalyzed differences of terminology. And it can hardly be disputed that such a work of classification would be favourable, if not essential, to the progress of science as a whole.¹

To this I would only add that the philosophers of science should be concerned no less with the clarification of theories themselves than with the clarification of the concepts occurring in them.

3 *What is the Philosophy of Science ?*

So far we have not defined the term 'philosophy of science', and many who study the subject believe that it is unwise to try to do so. Although I have some sympathy with their point of view, I believe that we are obliged to make the attempt, since we are often called upon by others to say exactly what we mean by the term. Moreover, although the definition of mathematics is not a question of mathematics nor are the definitions of physics or biology questions of physics or biology, the definition of philosophy is a question of philosophy. Likewise the question of its own definition falls within the scope of philosophy of science. Indeed, I believe that, in the long run, this question has to be faced for our response to it determines our whole approach to the subject.

Although I believe that the study of the philosophy of science can provide a useful antidote to increasing specialisation and the consequent fragmentation of science, this does not mean, as is sometimes stated and more often insinuated, that the subject is but the popularisation of science. Anyone who believes that should peruse Kant's *Critique of Pure Reason* or Reichenbach's *Philosophical Foundations of Quantum Mechanics*.

It is often stated that philosophy of science is the logic, or the analysis, of science. This definition, which is a highly reputable one, seems to me to suffer from two important defects—at least in the way in which it is often interpreted.

First, it tends to separate the scientist from the philosopher rather than to unite them, and so ultimately provides ammunition for those

¹ A. J. Ayer, *Language, Truth, and Logic*, London, 1948, p. 152

scientists who attack the subject. In particular, any attempt by a philosopher to axiomatise a particular branch of science usually results in an arid discipline of no value to the scientist, to whom it seems that the rigour imposed is *rigor mortis*, and who feels, in any case, that the philosopher is poaching on his preserve. That those who support this definition take the view that the philosopher of science should be distinct and have a different outlook from the scientist is clear from the pronouncement of one of the most distinguished, who maintains that

The philosopher of science is not much interested in the thought processes which lead to scientific discoveries ; he looks for a logical analysis of the completed theory, including the relationships establishing its validity. That is, he is not interested in the context of discovery, but in the context of justification.

And Reichenbach goes on to admit that ' the critical attitude may make a man incapable of discovery ; and as long as he is successful, the creative physicist may well prefer his creed to the logic of the analytic philosopher '.¹

Second, this definition of the philosophy of science tends to repeat, by implication rather than overt statement, the great mistake—common to Aristotle, Descartes, Kant and, I fear, many recent thinkers from Eddington at one extreme to radical empiricists at the other—of regarding science as something static, *sub specie aeternitatis*, rather than as something living and changing with the passage of time ; for, as Professor Dingle pointed out in his Inaugural Lecture at University College in 1947, ' the history of science is inseparable from science itself '.²

Consequently, I would suggest that it is far more satisfactory to regard the philosophy of science as a discipline which stands in roughly the same relation to the history of science (which for this purpose must, of course, include contemporary science) as theory stands to experiment in a science such as physics. This does not mean that it is the function of the philosopher of science to predict the future course of science. Indeed, strictly speaking, prediction in this sense is not the function of the theoretical scientist either ; when he predicts the result of an experiment he is usually making an assertion about an event which, in

¹ H. Reichenbach, *Albert Einstein : Philosopher-Scientist* (ed. P. A. Schilpp), Chicago, 1949, p. 292

² H. Dingle, *The Scientific Adventure*, London, 1952, p. 3

PHILOSOPHY OF SCIENCE

principle, could already have occurred. In other words, the prediction of an experimental result by a theoretical scientist, as distinct from an observational prediction by an astronomer, is not historical prediction. The principal function of the theoretical scientist is the critical study and systematic elucidation of the data provided by the experimental scientist. Similarly, the principal function of the philosopher of science is the critical study and systematic elucidation of the processes of scientific method, discovery, and explanation and of the particular habits of thought which the practice of science tends to encourage.¹ For the effective study of these processes and habits, data must be obtained from the historian as well as from the contemporary scientist. Moreover, as W. P. D. Wightman has stressed in his well-known book, *The Growth of Scientific Ideas*, Edinburgh, 1951, p. vii, 'a historical study undertaken in a critical spirit provides, I believe, a better introduction to "scientific method" than any abstract discussion of this difficult subject'.

'Science', as Professor Dingle has reminded us, 'may ignore its history, but if so it fails.'² *A fortiori*, I would say that 'the philosopher of science may ignore the history of science, but if so he fails'. For example, Kant's fallacious reasons for believing that the laws of motion were true *a priori* and Clerk Maxwell's belief in the law of inertia as a necessity of reason could only be held by thinkers who, in effect completely ignored the historical development of these laws.

When we pass from a securely established science such as physics to the far less securely established social sciences the significance of the historical perspective is perhaps even more apparent. As a recent writer on 'The Historian and the Philosophy of Science' has pointed out, these are 'pioneer disciplines and what they seek of philosophy is central guidance'.

[The social scientists] are faced with the task of devising a more exact methodology, and of placing the foundations of their sciences on a firmer base. To this end, a long range perspective of the nature and meaning of science is required, and such a perspective can be gained only through history.³

An outstanding problem which illustrates this interdependence of the philosophy and the history of science is the fascinating question of

¹ It is in this sense that I believe that the philosophy of science should be regarded as the analysis of science or 'metascience'.

² Ibid.

³ T. A. Cowan, *Isis*, 1947, 38, 17

how modern science originated. This is a problem which concerns the philosopher of science no less than the historian, for it goes to the very root of his discipline. It is coming to be increasingly recognised that explanations based on the assumption that this process began when, and only when, students of nature abandoned the library and cloister and became empirically rather than theoretically minded are at best extremely inadequate if not downright misleading. Indeed the Baconian view of science which usually characterises such accounts, that 'the true and lawful goal of science is the endowing of human life with new discoveries and powers', fails to provide any explanation of the puzzling fact that other highly intelligent civilisations, for example the Chinese, the Islamic, and the Byzantine, eagerly sought the same goal and yet completely failed to anticipate the scientific revolution of the sixteenth and seventeenth centuries in Western Europe.

The Byzantine failure is even more striking than either the Chinese or the Islamic, particularly as Byzantium was the direct cultural heir of Hellenistic civilisation. As a leading historian has pointed out, 'the Byzantine love of theory and culture, great and highly vaunted though it was, was sterile. It was unexpectedly in practical efficiency that their genius lay.'¹ They discovered the inflammable liquid known as 'Greek fire', perfected the dome in architecture, developed the Roman system of water supply and drainage, clocks and ingenious toys, 'the roaring lions and the soaring throne which made the palace so impressive to barbarians'. Medicine too was assiduously studied, but on the whole Byzantine medicine 'was admirable more for its common sense than for its theory'.²

In the light of this and other comparisons, the unique development of modern science in the West seems to have been the concomitant of the late medieval and renaissance criticism of Aristotle and to have been due primarily to men who were not just scientists and technologists but—and this is the essential point—were both scientist-technologists and philosophers of science at the same time. I believe that this historical fact, namely that the philosophy of science was—and, I maintain, still is³—an indispensable factor in the development of science

¹ S. Runciman, *Byzantine Civilization*, Cambridge, 1930, p. 239

² *Ibid.* p. 237

³ Since this Address was delivered no less a person than the President of the Royal Society, in his Address to the Convention on Digital Computer Techniques held in London at the Institute of Electrical Engineers, 9th to 14th April 1956, has 'emphasised

PHILOSOPHY OF SCIENCE

itself, is the supreme vindication of our subject and the ultimate justification for its continued study.

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that the philosophy, in this case of machine design, preceded the actual construction and thus was not an afterthought about results, as it sometimes is'. (See *Nature*, 1956, 177, 1069.)

TOWARDS A SCIENCE OF SOCIAL RELATIONS (II) ★

G. A. BIRKS

9 *Relation between the Components of the Creative Act*

WE have now isolated within the creative act two components, the first of which is of the nature of thinking, for it occurs entirely within the mind, while the second is of the nature of acting or doing, for it occurs entirely within the objective or physical world. Thus equipped we can now return to the initial problem as defined in Section 2, that of 'elucidating the conception of intelligent behaviour' by 'defining some constant relation between thought and action'.

The simplest case we have found to be conceivable is that in which the gestures involved in recognising Mr Y, and the time interval between recognition and stepping out, are both at their zero limit. According to our analysis we can distinguish in this case the following processes :

- (a) At first sight of Mr Y, Mr X recognises him, thus creating a new subjective world (thinking component).
- (b) At the same time he steps into the road to meet him, thus creating a new objective world (acting component).
- (c) Still at the same time, he is aware of this discontinuity in his own movements, thus creating a new subjective world (thinking component).

The most general case is that in which neither of the possible zero limits is operative. This is more complex :

- (a) First sight of Mr Y.
- (b) Mr X recognises Mr Y (thinking component, as above).
- (c) The recognition involves physical gestures (acting component).
- (d) Mr X is aware of these gestures (thinking component).
- (e) An interval, short or long, of uncreative activity, during which Mr X works out in thought the implications of the new situation, and considers new courses of action.
- (f) Mr X steps into the road (acting component).
- (g) Mr X is aware of this change of action (thinking component).

The two cases are not really much different, for in the more general case we see that (c), (d), and (e) do not affect Mr X's action, and do not help us to understand it, so that we are left with the same processes as in the special case, except that they are spread over a period of time. It will be sufficient therefore to examine the special case.

★ Part I of this paper appeared in the August number

TOWARDS A SCIENCE OF SOCIAL RELATIONS

The point I wish to emphasise is this. Even when all the processes are simultaneous, they fall into a logical order which, in the more general case, becomes largely a temporal order also; first the new situation, then the new action, then the new situation produced by this action. It would of course be much more neat and tidy if we could bring all the thinking components together and all the acting components together, but it would plainly be quite arbitrary to attempt it. *Thought is logically antecedent to action* (whether temporally antecedent or not), *and action is logically antecedent to awareness of action* (though never temporally antecedent).

Further than this I find it impossible to go. I have already definitely stated that the thinking component (the new subjective situation) is not the determining cause of the acting component (the change of action), for other courses of action were open to Mr X.

Another formula may, however, be useful. An outside observer has no direct access to what is passing in Mr X's mind (the thought component); but if he saw Mr X stop, gaze fixedly at a man across the street, and then step across the road in his direction, he might guess correctly Mr X's thoughts and intentions. On the other hand, he might guess incorrectly. Just as Mr X's new world opens up only a limited number of suitable courses of action, so to the onlooker Mr X's unexpected actions open up only a limited number of possible antecedent thought components, and among these he may choose correctly or incorrectly. We may say then that *the objective component is the imperfect expression of the subjective component*—the only expression available. Here perhaps is a basic principle for a theory of the communication of thoughts, though far removed from language and other symbolic forms of expression.

If now, applying the above principles, we seek a clearer conception of intelligent behaviour, we may say that intelligent behaviour is self-conscious action consistent with the whole situation as known (the subjective world). Self-consciousness is of course no objective criterion, but the remainder of the definition is perhaps adequate to distinguish intelligent behaviour from mechanical behaviour (which even when teleological is quite blind) and instinctive behaviour (which is a reaction to a particular stimulus, regardless of the general situation).

10 *Inertia*

So far we have been giving our attention to the crucial moments of the story, but the analysis raises some interesting problems concerning the intervening periods.

From the story it seems at first glance that these periods, since they contain no crucial moments, are marked by no creative acts, no discontinuity. But this cannot be so. If Mr X happened to recognise any other friend, and greeted him and passed on, such recognition, accompanied by appropriate action, would have differed in no fundamental respect from the recognition of Mr Y. Every time Mr X noticed anything not strictly predictable from his previous knowledge, every time he acted in any way not strictly predictable from his previous movements, there would be discontinuity both subjective and objective. The distinction between creative and non-creative acts no longer corresponds to that between crucial moments and intervening periods; Mr X's activity is uncreative only when his thoughts and actions are strictly predictable from their immediate antecedents.

If some creative activities seem more crucial in the story than others, that is plainly due to Mr X's purpose. He wished to speak to Mr Y; therefore his encounter with Mr Y had a special significance. In this way we find ourselves obliged to introduce the conception of value, but this will be set aside for the present, until purpose comes to be more fully examined.

In the meantime the story plainly relates that Mr X while walking along the pavement is more or less steadily pursuing a single course of action, a course intelligible in Mr X's initial subjective world. In spite of any creative activities that may have occurred in this period, Mr X evidently continued to live (i.e. both think and act) in a world which remained constant in all respects relevant to his purpose. Here is an example of continuity in spite of discontinuity, a continuity which persists even while creative acts are occurring. We know from experience that this continuity can even bridge over considerable periods of unconsciousness; for when we wake after long sleep, we know we are still the same persons, and we soon recognise the familiar surroundings.

In stressing the discontinuity of Mr X's worlds, I have hitherto ignored this element of continuity, yet it is plainly there. When Mr X saw Mr Y in the street, his new world was discontinuous with respect to the location of Mr Y, but not with respect to many other features, such as their houses. In this instance the discontinuity is so slight that we should ordinarily speak of the process as one of adjustment or correction, but it is not always so simple as this. In 1887 the Michelson-Morley experiment showed a discrepancy between the actual world and the world of classical physics. Nearly twenty years later a new physical world was devised, incorporating the Michelson-

TOWARDS A SCIENCE OF SOCIAL RELATIONS

Morley result with as much of the older system as could be retained without contradiction. If we are to do justice to the discontinuity as well as the continuity in Mr X's case, we shall not speak of correction or adjustment, but shall say that he too creates a new world incorporating his latest observation with as much as can be saved of the old world.

The tendency for motion to continue in the material world is called inertia. The tendency of knowledge or belief to persist may well be called psychological inertia. There is no need to consider continuity of action, for we have shown that this follows from the continuity of thought and purpose.

To account for the continuity between creative moments the following Principle of Psychological Inertia is sufficient : *Every human being continues to live in the same world as long as this is consistent with the objective world.*

To account for such continuity as persists in spite of creative activity, something more is necessary, and I provisionally put forward the following Principle of Minimum Discontinuity : *When compelled by observation of the actual world to create a new world, every human being incorporates into his new world as much of his old world as can be retained consistently with his new knowledge.*

II Recapitulation

This completes my analysis of situation ; purpose will have to wait for future treatment. The following recapitulation, section by section, may be helpful.

(1) There is scope for a new system of basic concepts relating to man's social behaviour.

(2) I propose to seek these concepts by analysing a simple human action at the rational level of behaviour.

(3) Commonsense regards an action as completely explained when the motive (or purpose) and the circumstances (or situation) are known. I adopt this approach, though I realise that on these terms human behaviour is strictly indeterminate.

(4) I proceed to an analysis of situation when purpose is constant. I distinguish between the objective world, as known to an omniscient observer, and the subjective world. To be intelligible, a man's actions must be referred to his subjective world.

(5) When a man's subjective world proves incompatible with observed facts, he changes it. The change is discontinuous. The

man thus lives in a discontinuous series of subjective worlds, punctuated by unexpected observations.

(6) This discontinuity is not observable to others. But if the new situation calls for a new course of action, and the man unpredictably changes what he is doing, discontinuity occurs in the objective world. The moment of objective discontinuity is generally, but not always, later than that of subjective discontinuity.

(7) Every discontinuous activity is a creative activity, creating (presumably) either a new subjective world, or a new objective world, or both.

(8) On closer examination it appears that either of these two forms of creative activity inseparably involves the other, though a zero limit is conceivable in one case. The dichotomy subjective-objective being perfect, every creative act at the rational level can be resolved into two components, a thinking component and a doing or acting component.

(9) The relations between these components are complex. The three essential processes involved fall naturally into a logical order in the sequence—thinking of the new situation (subjective), new action (objective), thinking of the new situation created by the new action (subjective). To any observer the objective component is the imperfect expression of the subjective component.

(10) To account for the continuity of thought and behaviour between moments of creative activity, a Principle of Psychological Inertia is necessary; and to account for such continuity as persists beyond the creative activities, a further Principle of Minimum Discontinuity is necessary.

12 *The Story Retold*

I propose finally to run through the story of Mr X once more, hoping in this way to show how much has been accomplished and how much remains. I begin before Mr X leaves home, and proceed by stages.

Preliminary. Mr X is actuated by the same purpose throughout—that of speaking to Mr Y. Initially he is living in a world in which Mr Y is assumed to be at home. Various intelligible courses are open to him—telephoning, walking, taking a taxi, etc.

First crucial activity. In setting out to walk, Mr X takes unpredictable action and thereby creates a new objective world. The subjective component of this act is his self-conscious awareness of the discontinuity of his own action.

TOWARDS A SCIENCE OF SOCIAL RELATIONS

Interval while walking along street. Whatever creative acts occur are irrelevant to Mr X's purpose. His belief that Mr Y is at home persists between and beyond these acts according to the Principles of Psychological Inertia and Minimum Discontinuity. His course of action therefore remains intelligible.

Second crucial act. Seeing Mr Y on the other pavement, Mr X becomes aware of a discrepancy between his world and the actual world, and creates a new subjective world. The objective component of this act is the discontinuity of his accompanying gestures, and self-conscious awareness of this takes its place in the subjective world.

Interval (if any) before stepping off pavement. No new world, subjective or objective, is created, so this is a period of psychological inertia. Mr X is adjusting himself to the new situation and considering possible intelligible courses of action—crossing, beckoning, etc. (The analysis throws no light on the apparently reflective thought processes involved at this stage, or indeed elsewhere.)

Third crucial act. In stepping off the pavement (one of the courses intelligible in the new world) Mr X takes unpredictable action and thereby creates a new objective world. The subjective component of this act arises as before from self-consciousness.

Interval while crossing the road. Principles of Psychological Inertia and Minimum Discontinuity again operative.

Social consequence of the third crucial act. Up to this point Mr X's behaviour has not been significant to other persons, each pursuing his own purpose in his own world. The car driver was living in a safe and orderly subjective world. A discrepancy arises when he sees Mr X unpredictably crossing the road. He creates a new subjective world, and takes intelligible action (objective component). A brief period of psychological inertia probably follows before the impact occurs.

Fourth crucial activity. The impact itself is a physical event, in which both Mr X and the driver are passive. But it makes Mr X aware of a discrepancy between his subjective world and the objective world, and presumably provokes a fourth creative act.

The analysis is not complete. Purpose remains to be considered, and—associated with it—value. Further, the above reconstruction breaks down when certain kinds of thought process are involved, as I have pointed out. No doubt there are also many tacit assumptions to be brought into the open. But the line of thought seems suggestive, and it may be that a platform for further operations has been established.

DO COMPUTERS THINK ? (II) *

MARIO BUNGE

6 *Do Machines Abstract ?*

At this point the cybernetician might step in arguing that, as there are levels of mathematical work, there are also stages in the development of machine-building, so that one cannot be sure that future artifacts will not surpass those of the analytic type. A reply to this argument could be : (a) No machine can ever attain the level of abstraction because machines merely represent abstract thought, but they do not handle abstract entities, nor, *a fortiori*, can they create new abstract objects, as they are secluded in the circle of inanimate matter, on which man can stamp his intelligence, but which lacks the material prerequisite to attain intelligence, namely life ; (b) is it not much easier and important to beget and to train normal mathematicians ?

Our hypothetical cybernetician would probably rejoin that, while it is true that computers so far built lack the capacity for abstraction, other machines have it. For example, the 'reading machine' designed by McCulloch and Pitts is said to have such a faculty : it is able to 'recognise' the same general shape, or pattern, in material objects having individual differences (e.g. printing-faces of different sizes and styles). Cyberneticians hold, in sum, that this machine 'recognises universals'.

The assignment of the faculty of abstraction depends, of course, on the meaning attributed to the word *abstraction*. Also, it is plain that cyberneticians use in this connection the common, non-technical acceptance of that word, namely the one according to which abstraction consists of taking away, dispensing with, taking aside. Now, it should be remembered that this is not the sole connotation of the word in question. Moreover, to employ the word 'abstraction' to denote such an operation is often misleading, for it applies not only to mental but also to physical processes. Indeed, on that definition of abstraction it might be said that the gravitational field has the faculty of abstracting in the highest degree, for it pulls all sorts of bodies, 'abstracting from',

* Part I of this paper appeared in the August Number

DO COMPUTERS THINK?

or dispensing with, their properties. Would it not be nonsense to hold that? My claim is that the same kind of 'abstraction' works in the so-called 'recognitive artifacts'—not, however, the faculty of performing the abstract operations of the synthetic and the critical types referred to above.

In fact, what is the mechanism by which 'recognitive artifacts' are said to 'perceive abstract forms'? Essentially it is the principle of specific sensitivity, which operates in wave filters, which 'recognise' whole groups, or bands, of frequencies. This is not too distant from the humble sieve used in the kitchen to separate bodies of different sizes. In all these cases only physical laws are involved, and not mysterious ones.

The claim that the 'reading machine' (i.e. the artifact that converts optical into acoustic signals) is able to abstract, might be justified on the empiricist doctrine of abstraction. According to traditional empiricism, abstraction is only taking away, setting aside, ignoring, or forgetting particulars—never adding anything new; for this school, abstraction is mere schematic representation in thought of facts of experience. This doctrine—shared by detractors of intelligence, like Bergson—may account for the first level of abstraction, the one characterised by generalisation through elimination of particulars. This is the kind of induction that dogs do when they put all cats in a single class; it is also the one we make when speaking of the cardinal number of a collection irrespectively of the nature of the elements of it. To this lowest level of abstraction, which we share with the higher animals, the usual definition of abstraction does apply. But this is not the sole level of abstraction attained by man, and the higher levels of abstraction are not entirely reducible to the lower, although they are rooted to it.

7 *Can Machines Outdo their Designers?*

Man is not only able to ignore or to forget—a privilege which he does not share with machines, which, not being able to know, are unable to ignore and to forget. Man is not only able to disregard individual characteristics concentrating on common traits; he is also able to invent new objects not suggested to him, at least directly, by experience. For example, when we speak of moving bodies in general, we stand on the first level of abstraction; but when we refer to bodies and to motion separately, we perform a sort of quartering

of sensibles, thus stepping on a higher level of abstraction. Again, when we introduce the concepts of actual infinity, irrational number, Riemann surface, vector potential, and the like, we create ideal objects lacking an empirical counterpart, although they may be correlated with experimental data through certain intermediaries; here we are moving on a third level of abstraction, the level of ideal objects not originated in simplification (first level) nor in quartering (second level). This third level of abstraction is characterised by new 'emergent' qualities—although the followers of the empiricist tradition maintain that even 'our most remote abstractions are all ultimately reducible to primitive atomic propositions and the calculus of the lowest level'¹ and that, in its turn, atomic propositions are nothing but peculiar nerve impulses.

Machines are not entitled to be even compared with their designers in the field of the higher levels of abstraction; as has been suggested above, some of them can 'recognise' universals of the first degree (e.g. *squareness*)—in the same sense as a home-made hygrometer, lacking a graduated scale, might be said to 'recognise' the universal *humidity*. The physical processes occurring in 'recognitive artifacts'—and also in non-recognitive ones—are the material correlates of abstraction of the first kind, or level. The same as in the case of computers, what is at stake is a material representation of a mental function, not the function itself.

Obviously, machines are usually built because they can do some things which man either can do but painfully or inaccurately (washing machines, differential analysers), or which he cannot do at all (air-planes, radio sets). In this sense they surpass their builders, thereby falsifying the scholastic maxim (adopted though not invented by Descartes) that there can be nothing in the effect that had not been in some way in the cause. But machines cannot surpass man in everything, even though we are told that the new computers 'are capable of learning and thinking far beyond us'.² For all their usefulness, machines are products of culture, whereas their designers are, besides, producers of culture objects. And, rigorously speaking, machines surpass nobody in nothing; what happens is that a given designer of machines may surpass some colleague of his by building an improved machine.

¹ W. S. McCulloch, 'Why is the Mind in the Head?', in M. Monnier (Ed.), *L'organisation des fonctions psychiques*, Neuchâtel, 1951, p. 38

² W. S. McCulloch, *this Journal*, 1954, 5, 18

DO COMPUTERS THINK?

Perfectibility is indeed a characteristic of living matter absent in machines. Perhaps some machines can 'learn' something, i.e. can make use of accumulated experience. But, (a) theirs is, so to speak, an individual perfectibility, since it is not transmitted to the species *machina ratiocinatrix* through reproduction nor through culture: it begins and ends in the individual machine; (b) machines do not seem to be able to advance in a sense very different from the way animals progress, namely by trial and error; this is, indeed, the behaviour typical of machines with self-correcting (feed-back) mechanisms, and is the least intelligent way of learning, because it is not planned and because it does not make use of another's experience; (c) rigorously speaking, machines do not 'learn' by themselves, but are 'taught', either by their designer or by external circumstances.¹

Man learns not only as an animal, i.e. through mistakes; he learns mainly through the agency of society, which acts on the biological and psychological mechanisms of learning. This is why man can dispense, to a large extent, with purely biological progress, advancing at a rate that is without a parallel among lower animals. One of the reasons why man covers levels of learning higher than the peculiarly animal level, is that he is endowed with consciousness: unlike the machine, man is able to know what he does, how he does it, and why he is doing it. Therefore man comes to know, among other things, that he must go forward in order to survive; and in some cases he is even able to discover that he himself deserves the credit for it.

8 Artificial Thought?

All machines save both mental and physical toil. But they do not always save work because they do it in our place. For example, a (new) car may save us the effort of walking, but not because it walks instead of us; the car performs a completely different operation, which amounts to walking only in so far as both motions have the *net result* of displacing our bodies over space. The same holds for computers and other 'machines that think': to assert that they think is as erroneous as saying that cars walk. Machines do not save us mental work because they do it, but in spite of the fact that they do something very different, which the designer has *correlated* with certain mental operations.

¹ M. V. Wilkes, 'Can Machines Think?' *Proceedings of the Institute of Radio Engineers*, 1953, **41**, 1230

In this very restricted sense, computing machines can be said to perform what has been called artificial thinking.¹ Not in the same sense as synthetic compounds, such as vitamins, are called synthetic, for the properties of the artificial and of the natural chemicals are often exactly the same, which is obviously not the case with artificial thought. (Think of the punched card yielded by a computer.) Machines can be said to perform artificial thinking in the same sense as cars can be said to perform artificial walking: because they yield net results which are equivalent to the model in a single respect—whereas in the case of synthetic compounds the identity often covers all known aspects of the end result.

Mays² has coined an irreplaceable formula for designing 'machines that think': he said that they *think by proxy*. The full meaning of this statement should be appreciated, especially since it is metaphorical. To say that digital computers think by proxy does not mean that they think only in a limited way, or lazily, or solely on command—not even that they think for us, nor for our sake. It means that they do not think at all, although they perform operations that represent our thought in a certain field, yielding results devoid of intellectual content but which, when translated into the language of ideas, can usefully be incorporated in reasoning. To marry by proxy may have a legal value, but no more than this; the same happens in connection with machines: man does not delegate thought to the computer for the very simple reason that the computer cannot think, but can instead perform functions which we correlate with thought. Analogously, a portrait may represent a person, but it is not a person; to confuse both may lead to magic.

The computer, like every other automatic machine, runs for our sake; it would be wrong to infer from this that it acts *as we*. To commit this fallacy—and most cyberneticians incur in it—is the same as to confuse the deputy with the deputised thing. This is what people do when they confuse a piano-player with a pianist, or the vicar of God with God. This fallacy, of inferring that something acting for us must in some way or other participate in human nature, is typical of primitive and archaic logic: it is called reasoning by participation, and is the kernel of magic rituals. To conclude essential kinship in nature from mere correlation, from resemblance in pattern,

¹ P. de Latil, *La pensée artificielle*, Paris, 1953

² W. Mays, 'The Hypothesis of Cybernetics', this *Journal*, 1951, 2, 249

DO COMPUTERS THINK?

is to push analogies too far ; so far, that the difference between science and magic is lost. Needless to say, science began when the very procedure of cybernetics, namely magical play with anthropomorphic analogies and with metaphors, was rejected.

9 *Metaphors and their Misuse*

A distinctive mark of cyberneticians is their love of metaphors. Thus, they use to say that artifacts think, know, receive and give information, learn, wish, and even get sick. This is one of the main troubles with cybernetics, namely, that it does not distinguish between identity and resemblance, between the model and the portrait ; that, in short, it uses key concepts in wrong contexts. When a whole science and a whole philosophical literature are built on linguistic traps, one is entitled to distrust the slogans by means of which the new faith is advertised—or, at least, one has the right of smiling at Wiener's warnings¹ against that very use of concepts out of their proper context, in which he systematically indulges.

However, if the confusion of somebody with its deputy may lead to nonsense, it would be equally foolish not to realise that sometimes there may be something in analogies. Two very different objects may have something in common at some level or in some respect—and usually material objects do have a lot of sides in common. To realise this is as important as to avoid concluding sharing of essentials from mere resemblance in particulars or even from mere correlation, i.e. from similarity in structure. For example, memory in computers and in man are assuredly totally different at the physiological and at the psychological levels, at which machines do not even exist ; but there is a similarity of pattern at the physical level, for what is properly called memory in the case of higher animals, and improperly so in the case of artifacts (where it might be called 'storing'), is the capacity for retaining or storing some condition. Not to recognise such general traits shared in common, or likenesses in pattern, may lead us to support dualism or idealism with regard to the mind-body problem, thus favouring the return of the much discredited philosophical psychology, still in vogue in Germany and its philosophical dependencies. But to claim that partial identities and formal resemblances are *all* that matters—holding, for example, that machines can store ideas—

¹ N. Wiener, 'Some Maxims for Biologists and Psychologists', in M. Monnier (Ed.), *L'organisation des fonctions psychiques*, Neuchâtel, 1951

is to push analogies so far that their heuristic function becomes lost sight of.

Now the whole of cybernetics is built on such physiological and psychological analogies. The fact that some of them are deep lends it strength; the fact that they are nothing but analogies deprives cybernetics of methodological solidity—at least in the opinion of those who do not accept the philosophy of the *as if*. The great merit of cybernetics lies, in my opinion, in having pointed out and worked out something which was far from new but which is true, namely, the physical basis of life and mind functions. The main shortcomings of cybernetics are probably, (a) to have proclaimed that life and thought have no such physical basis, for they *are* just physical phenomena (mechanistic levelling down), and (b) to have levelled computers up to the level of the human nervous system (animistic reduction).

The levelling down is effected by means of what has been regarded¹ as the central hypothesis of cybernetics; according to it, the essential mechanism of the nervous system is a purely physical one, namely negative feed-back. The levelling up lies in the claim that there is no distinction in principle between the observable behaviour of a suitably designed artifact, and the behaviour of the human brain.² This peculiar blend of animism and mechanism, which characterises cybernetics, might be called *animechanism* or, as has recently been proposed,³ *technozoism*.

To say it in fewer words, the positive contribution of cybernetics consists, in my opinion, in its emphasis on the existence of connections between levels the very existence of which it denies, namely the physical, the biological, the psychological, the intellectual, and the cultural levels.

10 Summary and Conclusions

To sum up, we may say that computers count, add, etc., at the physical level, performing operations that are usually not regarded as mathematical (at least by mathematicians), since mathematics, an abstract science, is not interested in cogwheels, relays, electron tubes, electric pulses, etc. It is we who frame a correspondence (when building, 'feeding' and reading the machine) between the concrete

¹ J. O. Wisdom, 'The Hypothesis of Cybernetics', this *Journal*, 1951, 2, 1

² D. M. MacKay, 'Mindlike Behaviour in Artefacts', this *Journal*, 1951, 2, 105

³ H. Rodríguez, 'Cibernética y Pensamiento Humano', communicated to the International Congress of Philosophy of São Paulo (August 1954)

DO COMPUTERS THINK?

objects handled by the computer and our abstract objects. Without the human initial and final work of translating abstract into concrete objects back and forth, i.e. without the work of coding and decoding, the best of computers is helpless. In this respect, highly automatic machines do not differ essentially from the modest pencil, the simple abacus, or the cheap desk-computer, even though they are essentially different from the technological point of view.

Strictly speaking, computers do not compute, machines do not think, but they perform certain physical operations that we co-ordinate with certain mental processes. Since co-ordination, or one-to-one correspondence, defines identity of pattern, the whole resemblance between machines and man is an identity of pattern, a formal identity or isomorphism of some of the operations of the machine and a small section of human activity. Without the intervention of man's abstract and purposive activity, which has no counterpart in machines, the most expensive digital computer is mere scrap iron.

Machines, however automatic, are tools, that is, material assistants of man. To hold that they compute, think, know, learn, or wish, without specifying that this is only a metaphorical way of speaking ; to forget that machines *represent* some mental functions at the level of technology without performing them ; and to forget that these deputies of ours act only on command, whether immediate or long-run, is to confuse resemblance with identity, the part with the whole, the form with the essence, thus incurring in magical thinking. Those who write of the *machina ratiocinatrix* may astound the layman, *épater le bourgeois*, or delight the dilettante ; but by so doing they hardly deserve to be called the upholders of a tradition of scientific earnestness.

Modern artifacts are marvels of ingenuity, but they are not human and they behave not as humans : if they did, we would not use them ; artifacts are peculiar physical systems organised by technology to serve man. Is this not enough ? Why should the merits of their designers be attributed to them ? Why should men 'imagine, not only the forms of the gods, but their ways of life to be like their own' ? Are there not enough idols without that ?

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SOME ASPECTS OF PROBABILITY AND INDUCTION (I)*

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MR WILLIAM KNEALE'S *Probability and Induction* is one of the best modern books of its kind, and well deserves the high place which it now holds in contemporary literature on the philosophy of science. It appears to be the case, however, that the warmth of the welcome accorded to it by reviewers has lulled the latter into allowing to pass without comment several points on which Mr Kneale is fairly clearly mistaken. Because *Probability and Induction* is much too valuable and important for it to be a safe or suitable repository of error, the present paper has been written in an attempt to set some of these matters to rights. Moreover, the three basic issues selected for discussion have an importance outside the place they have in Mr Kneale's book ; it is desirable, therefore, to enquire into them and also into certain general questions that arise out of them.

I *The Consilience of Inductions*

(i) In his discussion of the relationship between primary and secondary induction (i.e. between the formation of causal generalisations of 'first order', so to speak, and broader generalisations which bind together and explain a number of generalisations of the former kind), Kneale errs in the direction of over-simplification : at first on a small point, but later in a way which leads him to attribute to Whewell's doctrine of the consilience of inductions an importance which it does not merit and a degree of certainty which it does not possess.

The essential simplification consists in the treatment of the relationship between broader and narrower inductive generalisations as though it were always one simply of logical entailment. This assumption, together with the undisputed fact that if one proposition logically implies another then the latter is at least as probable as the former,

* Received 5. xi. 54

accounts for the crucial passage which I have italicised in Kneale's account of the matter. On page 107 he says :

. . . we can sometimes explain empirical generalisations in biology by showing that they follow from certain physical and chemical laws which are already accepted. When such an explanation has been given, the probability of the biological generalisation may very well be greater than it was before. For *the biological generalisation cannot now be less probable than the physical and chemical laws from which it is seen to follow*, and, since these laws, being of greater generality, have presumably been confirmed in many more instances than those which provide evidence for the biological generalisation, it is reasonable to suppose that their probability may be greater than that which the biological generalisation has attained before the explanation.

Thus, Kneale concludes, the bringing of a biological generalisation under a physical or chemical one actually increases its probability. And this he takes to be general : the example just cited concerns the relations between two sets of primary generalisations, not those between a primary and a secondary set ; but on Kneale's view the 'explanation' relationship in this case is fundamentally the same as that between, say, a set of generalisations about the properties of gases on the one hand and the kinetic theory of heat on the other.

Now the first difficulty here is to think of a concrete example of a biological law which *follows logically* from laws of chemistry and physics. One would have thought that a biological generalisation—as distinct from a chemical generalisation, which is of interest and importance to biologists—would contain some mention of biological subject-matter. And *this* could be derived logically from statements which mention only the subject-matter of chemistry, only if biological terms were defined by means of chemical terms, a project which is of course neither practically possible nor theoretically desirable.

What does happen, when it appears possible to explain biological generalisations in terms of physical and chemical ones, is that the biologist produces not definitions but *statements* regarding the physical and chemical make-up of his material, such that these statements, together with the physical and chemical laws concerned, entail the biological generalisation whose explanation is required. The relevance of the propositions of one science to those of another is not inherent in the meanings of the terms used by the two sciences : when relevance is suspected, hard work must be done to establish that the subject-matter under consideration in the one is the kind of thing covered by

the putatively relevant propositions in the other. All this is just to say that a Figure I syllogism requires a minor premiss; and this fact lets in a possibility of error which puts the generalisation to be explained on an entirely different level from the generalisation explaining it. To state the point precisely, and in relation to the quotation given above: it simply is not true that 'the biological generalisation cannot now be less probable than the physical and chemical laws from which it is seen to follow'. This is untrue because, in the only sense of 'follow' in which the example has any significant application in science, it might well be the case that one generalisation is true and another which 'follows' from it false, the laws of logic being saved by the falsity of the bridging proposition which establishes the relevance of the one to the other. It cannot be concluded from this, of course, that the explaining generalisation is always *more* probable than the explained; the situation is rather that no simple probability-relation of any kind can in this way be established between them. But this alone is sufficient to invalidate Kneale's conclusion that the finding of an explaining generalisation automatically increases the probability of the explained one.

(ii) When Kneale applies these principles to the relationship between a 'transcendent hypothesis' (i.e. the result of a secondary induction, such as the kinetic theory of heat or the corpuscular theory of light, which by the nature of its subject-matter cannot be verified by direct experience) and the primary inductive generalisations which fall under it, he escapes the above criticisms only to invite objections which are related but take a somewhat different form. Consider, he says, a set of three supposed laws, L_1 , L_2 , and L_3 , and a transcendent hypothesis, H , from which they all follow. Considering these four together, we can say definitely that L_1 , L_2 , and L_3 are no less, and may well be more, probable than H ; for they follow logically from H , and are not false unless H is false also.

This time no objection will be raised at this stage of the argument. For, while in the previous example it was objected that the chemical composition of living things is neither incorporated in their definitions nor forms part of chemistry, here we may admit that the *application* of the concepts introduced by the transcendent hypothesis is specified by the transcendent hypothesis itself. That is, it does seem reasonable to say that the transcendent hypothesis known as 'the kinetic theory of heat' includes statements to the effect that such-and-such sensible phenomena *are* the temperature, pressure, etc., spoken of in the hypo-

thesis. The difference between this and the earlier case is not just one between an explaining hypothesis which is part of the same science as the explained one, and an explaining hypothesis which is part of a different science from the explained one—such a distinction would be largely verbal. What we have here is rather a distinction between the case where two sets of generalisations each arise independently and directly out of observed facts, and the case where one generalisation or set of generalisations is advanced purely because the *other* set has arisen out of the facts and where the former introduces concepts not already in use and therefore suitable for more or less arbitrary application to perceptual situations.

We allow, then, the claim that L_1 , L_2 , and L_3 (hereafter called the '*L*-laws') are more probable than H . But, Kneale continues, consider the situation before H is applied to the *L*-laws; consider, that is, their relative probabilities in the situation when the evidence for H is what it always was and always will be, but when the evidence for the *L*-laws is only what it was before H was thought of. At this stage of the proceedings, *H is more probable than the L-laws*. And, since nothing happens to the probability of H by its being 'applied' to the *L*-laws, we must say that something has happened to the probability of the *L*-laws themselves if we are to account for the fact that they begin with a probability smaller than that of H and end, after being 'explained' by H , with a probability greater than that of H . And this, Kneale claims, is the consilience of inductions of which Whewell wrote: in essence, it is the doctrine that inductive generalisations gain in probability according as hypotheses can be found from which several of them follow.

This argument has about it a compelling air of sleight-of-hand: it is just too good to be true. The mistake lies in the italicised clause in the above paragraph. What evidence does Kneale produce to support his view that if the *L*-laws follow from H , then H is more probable than the *L*-laws if we count all the available evidence in favour of the latter except H itself? 'The evidence in favour of H ', he says, 'is all the evidence in favour of all the consequences that follow from it, and in relation to this mass of evidence H may well attain a higher degree of probability than any one of its consequences, L_1 , L_2 , and L_3 , had in relation to its own special range of evidence before it was explained' (p. 108). This, however, in no way yields the conclusion desired; for, by just so much as H is in a position to have more evidence in its favour than have the *L*-laws, by so much

does H say more than do the L -laws, and so by that much is H more liable to falsification. If H logically implies the L -laws, then it is not more probable than they, and this fact cannot be avoided by talking about the 'application' of H to the L -laws: if it implies them, it implies them, and from this the fact about probability follows.

What is logically the same objection may be made to what is logically the same mistake, when Kneale offers what is almost his only other remark in support of the view that the finding of H increases the probability of the L -laws: 'After the explanation L_1 , L_2 , and L_3 may therefore be more probable than they were before, because each of them derives support indirectly from the evidence in favour of each of the others' (p. 108). That is to say that, given three propositions p , q and r , which appear to have nothing to do with one another, if we can find a fourth proposition, s , which implies them all and which we do not know to be false, then evidence in favour of p becomes evidence 'indirectly' in favour of q and r . It is true, with qualifications, that if s implies q , then q 's truth raises the probability of s 's truth. This is a special case of the fact that if s 's truth raises q 's probability (a general circumstance of which s 's implication of q is a kind of degenerate case) then q 's truth raises s 's probability. But this fact affords no support for Kneale's account of the consilience of inductions. For even if we grant that the assumption of L_1 's truth raises the probability of H , it is not necessarily true that *this* rise in the probability of H carries with it a rise in the probability of L_2 and L_3 . For example, if on certain evidence 'Patrick and John are both Scottish' has a certain probability, then the addition of 'Patrick is Scottish' to the evidence will, if it be a genuine addition, increase the probability of 'Patrick and John are both Scottish'; but *this* rise will not affect the probability of 'John is Scottish', unless the evidence already includes the establishment of some connection between Patrick's Scottishness and that of John. Now in the scientific case we have, *ex hypothesi*, established no connections between the L -laws other than their following from H . The raising of the probability of an L -law may increase H 's probability, but only in so far as H concerns that L -law. To assume that this rise of probability in H can affect the probabilities of the other L -laws is to assume that there is some connection between them *apart from* the fact that they are all consequences of H ; and the point of the whole procedure is that this is just what we do not know.

All this is not to deny the doctrine of the consilience of inductions: there clearly is a sense in which particular statements of any science

gain in probability when they are incorporated in *some* kinds of wider theory ; but only some, for a conjunction of the particular statements together with, say, ' Grass is green ' is a ' wider theory ' in some sense of that phrase ; certainly in a sense adequate for the whole of Kneale's argument. But the truth behind this fact is Keynes's theory of eliminative induction—Kneale's flat rejection of which will be discussed in the third part of this article—and it has nothing to do with the manipulation of probabilities simply on the basis of implication-relations.

2 *The Range Theory of Probability*

(i) No one would wish to quarrel with Kneale's contention that when the α 's and the β 's form two closed classes—i.e. when a definite number can be assigned as the number of the α 's, another as the number of the β 's, and a third as the number of the $\alpha\beta$'s—then, given that a thing is an α , the probability that it is a β is the fraction given by the division of the number of $\alpha\beta$'s by the number of α 's. The probability that someone is an American, given that he is an Oxford graduate, is the fraction yielded by the division of the number of Americans who are Oxford graduates by the number of Oxford graduates.

The trouble starts when Kneale comes to discuss the question of probability-statements regarding open classes where there is not even the theoretical possibility of comparing numbers as we can in the case of closed classes. We could, he points out, try to handle the matter in terms of the number of ways in which a thing can be an α , and the proportion of these which are also ways of being a β . For, since it would seem at first sight that an infinity of things might be α 's in the same way, this procedure may give us finite, definite numbers as a basis for our probability-fractions, even when we do not and cannot know how many individual members there are in each of the classes with which we are concerned. As it stands, Kneale is quick to point out, this proposal will not do : for we can make ' way ' as specific as we like and ultimately—on the plausible Leibnizian assumption—have as many ways as there are α 's and so still find ourselves dealing with unknowns or infinites. He therefore advances an ingenious method for grouping the ' ways ' in which a thing may be an α into sets which are ' equal ' in the sense that the members of each bear a special sort of one-one relation to the members of each of the others—a *special* sort of one-one relation because if any one-one relation

were allowed then an infinite set of possibilities would be equal to some of its sub-sets and the probability-theory erected on this basis would collapse. These equal sets of possibilities are obtained as follows. We call two characteristics 'independent' if neither necessitates the other and neither excludes the other. Now, if we take as our basic ways of being an α the complex characteristics α_1 , α_2 , α_3 , . . . etc., such that these alternatives can be specified further only by the addition of further characteristics which are independent of all these alternatives alike, then for each characteristic which we add to any one alternative we can add one characteristic—namely, the same one—to each other alternative. Thus we have our one-one relation: the relation expressed by '— is constituted by the conjunction of the same characteristics as —, except for . . .' and then follows a more or less involved specification of the way in which the original sets of alternative possibilities (hereafter called 'S-alternatives') were arrived at. These S-alternatives, then, carve up the totality of possible ways of being an α into equipossible chunks, do not themselves dissolve down into mere specifications of the individual members of open classes, and therefore give us definite, finite, numerical material on which to work in specifying probabilities. We say, in these terms, that the probability of an α 's being a β is the number of S-alternative ways of being an $\alpha\beta$ divided by the number of S-alternative ways of being an α . There are further complications in cases where there is admittedly an infinity of S-alternatives, but where they can be grouped off in a way analogous to the dividing up of a line (an infinity of points) into equal portions. These problems Kneale handles impeccably; our present concern is with less obvious but more radical difficulties.

This, in brief, is Kneale's version of the range theory of probability—though expounded with far less precision, detail, and subtlety than he devotes to it. It will here be argued that the theory is probably unworkable in all but the very simplest cases, and that, even if workable, it would not provide an adequate explicans of the concept of probability.

The question of workability hinges on the difficulty of finding S-alternatives which satisfy the conditions demanded without being so specific as to be the ultimate completely determinate alternatives which are in fact individual concepts and which, as Kneale points out, leave us with all the original difficulties of trying to handle open classes in terms of the numbers of their individual members. We may indeed be able to define—or at least to be able to envisage the possibility

of defining—a suitable set of S -alternatives under a given characteristic, say α , such that we can think of no further characteristic which cannot be indifferently added or not added to any of the S -alternatives thus arrived at. But the process is a good deal more difficult than it at first sight appears. Suppose, for example, that to what seems like a properly formed set of S -alternatives, $\alpha_1, \alpha_2, \alpha_3, \dots$ etc., we add a handful of characteristics $\beta, \gamma, \delta, \epsilon, \dots$ etc., to get the complexes $\alpha_1\beta\gamma\delta \dots, \alpha_2\beta\gamma\delta \dots, \alpha_3\beta\gamma\delta \dots$, etc. Can we still feel so confident that all remaining characteristics will be independent, in Kneale's sense, of *these* alternatives? It may be the case that ϕ , say, is independent of $\alpha_1, \alpha_2, \alpha_3, \dots$ etc., but this does not guarantee that it will be independent of, say, $\alpha_1\delta\gamma$ or $\alpha_3\beta\epsilon$. Yet it is precisely this which Kneale's theory requires; for if, after some further addition of characteristics to the S -alternatives according to the one-for-one rule, a characteristic turns up which is inconsistent *with some but not with all* of the complexes thus formed, then the one-for-one procedure breaks down and it can no longer be claimed that the S -alternatives are 'equal' in the sense required. And in any case, a complex of characteristics is itself a characteristic: if the characteristic ϕ is not independent of the complex $\alpha_3\beta\epsilon$, then the characteristic $\beta\epsilon\phi$ is not independent of the S -alternative α_3 . So independence of this very thorough sort must be demanded.

But careful consideration of what this involves suggests strongly that S -alternatives just are not specifiable in practice, and are not specifiable even in theory except as indefinitely complex, ultimate alternatives—individual concepts, in fact. For Kneale is using 'independence' as a logico-causal concept, a combination made possible by his doctrine—persuasively argued and tentatively adopted in *Probability and Induction*—that causal laws are 'principles of necessitation' of the same kind as are involved in logical necessity. Since this is so we must interpret S -alternatives as being sets of characteristics such that none of them has causal consequences not shared by all the others; and, since the relation between a perceived object and the percipient is a causal one which is necessitated by the object's other characteristics, the fact that an object has a disposition to behave as it does in a perceptual situation is a characteristic which is not independent of the object's other characteristics and which must therefore be incorporated in the S -alternative under which it falls—unless this disposition is shared by all objects of what for the purposes of the S -alternative in question may be called 'the same kind'.

From this it follows that the *S*-alternatives under a given general kind are at least as numerous as the things of that kind which, theoretically, we could come perceptually to discover and discern from one another. And there is no reason for thinking that in most cases it would not be theoretically possible to discover and distinguish each object of a given kind, quite apart from considerations of spatial and temporal separateness. The situation may be summed up thus: If we took the identity of indiscernibles absolutely seriously we should say that the above argument has as its conclusion that the *S*-alternatives of each kind are at least as numerous as the things of that kind; but, with a caution arising from the fact that here 'discernible' must be taken to mean 'theoretically capable of being *perceptually* discerned', we shall say only that in *most* cases the *S*-alternatives are at least as numerous as the objects falling under them, and that in *all* cases they are, if less numerous at all than the objects falling under them, less numerous by an amount which is completely indeterminate. So that in no case can we reach that finitude and definiteness which were the whole aim of the introduction of the theory in the case of open classes.

(ii) The second fundamental objection to the range theory of probability concerns its adequacy as an explicans of anything like the concept of probability as it is usually understood. Granted that there is no one correct way of using 'probability', it still remains true that the notion is linked in practically all its uses to the notions of frequency, chances of being right or wrong, best bets, and so on. This is the truth which finds its most convincing expression in the frequency theory of probability; and though Kneale finds good grounds for attacking some formulations of the latter theory, he too recognises the close link between probability and frequency, both in his identification of the two in the case of closed classes and in his admission that 'The frequency theorists are right in maintaining that [probability-statements] are inferred from observed frequencies' (p. 193). 'But', he goes on, 'they are wrong in maintaining that probability is to be defined in terms of frequency, and their error is the same as that of the philosophers who advocate the constancy theory of natural laws, namely, that of confounding evidence with that for which it is evidence.' In the case of probability, 'that for which [frequency] is evidence' is the relative proportions of what have here been called '*S*-alternatives'. Frequency, in short, is admitted to be a guide to probability, but it is the range theory which gives the *meaning* of 'probable'.

SOME ASPECTS OF PROBABILITY AND INDUCTION

Suppose for present purposes that there is some radical error in the first half of this section, and that in fact *S*-alternatives *can* be found which satisfy the required conditions without themselves being so numerous or so numerically indefinite as to be useless. Two questions may still be asked: What warrant have we for assuming that frequencies are in any way at all a guide to probabilities in Kneale's sense? And if there is indeed a connection between relative frequencies and relative numbers of *S*-alternatives, is it not the case that we are interested in the latter only in so far as they offer a guide to the former, and does this not suggest that the workaday concept of probability is concerned directly with frequencies rather than with the more esoteric facts which are of interest only in so far as (we shall provisionally suppose) they offer some clue as to frequencies?

As regards the first question, not much can be said beyond pointing out that for there to be any fruitful connection between frequency and probability (in Kneale's sense of 'probability'), it would have to be the case that the members of any given open class are distributed approximately evenly throughout all the equal alternatives. That is, the α 's will be divisible into a number of groups, each with about the same number of members, and the groups all being 'equipossible' in the sense in which equipossibility follows from their being defined by Kneale's *S*-alternatives. There are no special reasons for denying that the α 's, whatever they are, are so distributed; but there is equally no reason for affirming that they are. It *might* be the case that the universe is constructed on some principle such that two groups within a certain class have different numbers of members if and only if they are defined by characteristics embracing different numbers of *S*-alternatives; but then it might not. The situation finds a homely analogy in one of those little trays containing some half-dozen depressions into which a cake-mixture is poured before the whole tray is put into the oven. If there is more than enough cake mixture, of sufficiently high viscosity, to fill all the cavities, then however it is poured the cavities will all be filled. If there is less than enough mixture to fill them all, the cook *might* take the trouble to ensure that it is distributed evenly over all the depressions; but, on the other hand, he might just pour it in any old how, or he might prefer to fill as many as possible leaving one empty and one-half-empty. . . . The possibilities are legion, and we just do not have the requisite information about how the universe was poured into its original possibility-moulds.

With regard to the second question: Why should the language

even the bookmakers speak contain a word which means 'relative proportions of S -alternatives'? Without going into the whole complex of questions surrounding the problem of the nature and status of conceptual analysis, and admitting that such analysis may be more than just a report on ordinary usage, we must nevertheless insist that an analysis worthy of the name must retain some solid contact with the way in which the 'analysed' notion is normally used. And, surely, what matters to us when we think of probability is how often what kinds of things are going to turn up. If it *were* established that there is no simple correlation between relative frequencies and proportions of S -alternatives, surely not even the hardest range-theorist would retain his theory, with only a passing sigh for the loss of one means of discovering how S -alternatives are distributed.

If we have no information on frequencies, and if we can nevertheless work out some kind of system of S -alternatives for the case in hand, then by all means let us proceed on the basis of the latter. They are better than nothing, and will tally with actual frequencies at least to some slight extent, e.g. in settling whether or not there are limiting cases of the form 'All α 's are β 's' or 'No α 's are β 's'—though the method of S -alternatives will not tell us even this much in most cases. But to say that this is what really concerns us when we talk about 'probability' is just to show that one has not been listening.

(to be concluded)

NOTES AND COMMENTS

Comments on 'The Definition of Psychosomatic Disorder'

MAY I offer some critical comments on Nigel Walker's¹ recent essay on this subject? My purpose is to show that while I agree with Walker on his view of the difficulties which are inherent to the usual medical and psychiatric ways of dealing with the mind-body dichotomy, it seems to me that the definition which he offers avoids this problem by ignoring the subject matter of 'psychosomatic medicine' altogether and focuses instead on the skills of the therapist. Moreover, Walker fails to cite those authors who have emphasised the philosophically unsound character of any attempt to isolate a clinical 'entity' which could be labelled 'psychosomatic'.

Walker suggests what he calls a 'technician's definition' of psychosomatic disorder:

Psychosomatic disorders are somatic symptoms which can be successfully treated by methods effective in treating psychic symptoms (p. 295).

In line with this functional definition, he later adds that for the good psychotherapist, therefore, there will be many psychosomatic disorders, and for the poor one few (p. 298).

Let me indicate briefly the difficulties into which such an approach—which seems to me almost a caricature of operationalism—leads us.

(1) What are 'somatic symptoms'? A symptom, as this term is customarily used in medicine, is always something 'subjective' of which the patient complains. In contrast, physicians speak of 'signs' to denote phenomena which they observe and of which the patient may or may not complain; for example, the presence of unequal pupils in a case of head injury. What, then, does Walker mean when he speaks of 'somatic symptoms'? Clearly, he means symptoms which *refer to the body*, rather than to some object other than the body (e.g. other people, inanimate objects, etc.). This inference follows from his concept of 'psychic symptoms' of which he gives the following example: 'If I see a dagger in the air when nobody else does, because I have made up my mind to kill my king, this is a psychic symptom from a psychic cause' (p. 267). But suppose that the person experiencing such a 'delusion' is found to have, with the aid of the appropriate methods of study, a syphilitic infection of the brain. Does he then have this symptom *because* of somatic causes?

¹ This *Journal*, 1956, 6, 265

This is surely an outmoded view and one that does not fit even approximately the observed phenomena (such as the rôle of personal history as a determinant in all symptoms, irrespective of their 'necessary' causes). Would it not be more accurate to view all *symptoms* as 'psychic' and to introduce the additional concept of the *validation* of the symptom between patient and physician (or others)? Thus, if a patient who complains of so-called ulcer-pain is found by his physician to have (that is, to show 'objective' evidences of) a duodenal ulcer, then, according to Walker—and also according to common medical usage—he *will have* a 'somatic symptom with somatic causes' (or, perhaps, 'with some somatic and some psychic causes'). What is done in such and similar attempts to classify symptoms is not so much an ordering and clarification of the patient's experience but rather its validation, or the lack of it, by the observer. I do not want to labour this matter of validation any further here and would like to refer the interested reader to two articles in which I have developed this theme in some detail.¹ I might add, however, that Walker himself gives evidence of the rôle of validation in his classification of symptoms, without, however, making it explicit, when he refers to nobody else seeing the dagger, in the example quoted above. By implication, if others were to see the dagger also, he would be driven to some conclusion other than the one he proposes, such as perhaps that the perception is 'correct' and not a 'symptom' at all.

(2) The second half of Walker's definition, namely, that the symptoms in question can be 'successfully treated by methods effective in treating psychic symptoms' seems to me equally open to criticism. Objections to this formulation derive from two separate lines of thought. First, beginning with Freud's abandonment of the hypnotic ablation of *symptoms* and his early work with what was to become the psycho-analytic method, the notion of what constitutes 'improvement' in psychological 'illness' has become vague and variable, and such that comparisons with medical treatments can no longer be made in a meaningful manner. Disappearance of symptoms (and signs) is the crucial criterion of cure in 'organic medicine'; this is not true in psycho-analytic treatments where, instead, the concepts of 'working through', alterations in the transference relationship, and other concepts are used to gauge 'improvement'. The second objection to this definition is that in the everyday (empirical, symptomatic) sense of improvement, *all* somatic symptoms may be successfully treated, at times and by certain individuals, by 'methods effective in treating psychic symptoms', that is, for example, by psychotherapy. Adherence to Walker's definition would lead us to conclude that Jesus Christ and Mary Baker Eddy were

¹ T. S. Szasz, 'The Ego, the Body, and Pain', *J. Amer. Psychoanalyt. Assoc.*, 1955, 3, 177; 'The Nature of Pain', *A.M.A. Arch. Neurol. and Psychiat.*, 1955, 74, 174

THE DEFINITION OF PSYCHOSOMATIC DISORDER

not only successful psychotherapists—an opinion which is widely held—but also that they have dealt with huge numbers of ‘psychosomatic disorders’. If we agree that all modes of behaviour can be *influenced* by either psychological or physical (physiological) means, and further that the notions of improvement and cure—just as ‘disease’—are matters of judgment and valuation¹ and not simply a matter of observation, then it becomes rather dubious how such a therapeutically oriented definition as that proposed by Walker can be of much scientific value.

The foregoing considerations touch on the assertion that I made at the outset, namely, that several of the outstanding workers in this field have stressed the fallacy of labelling *any* symptom or disorder as ‘psychosomatic’. Here is what Alexander said :

The attempt to single out certain diseases as psychosomatic is erroneous and futile. Every disease is psychosomatic because both psychological and somatic factors have a part in its cause and influence its course. This assumption is valid even for such specific infectious diseases as tuberculosis.²

More recently, Grinker stated :

After World War II, many physicians sought training within a hypothetical specialty of psychosomatic medicine, unaware that ‘psychosomatic’ means a conceptual approach to *relationships*, not new physiological or psychological theories or new therapeutic approaches to illness.³

While it may be said that other writings of these, and of other, authors do not always live up to the foregoing platform regarding the nature and scope of psychosomatic medicine, it appears that in trying to define a ‘psychosomatic disorder’ to everyone’s satisfaction, Walker has chosen a task which many serious students have considered a pseudo-problem of this particular area of scientific interest. Perhaps the analogy between psychosomatic medicine and biological chemistry is not too far-fetched: both *combine* the concepts and methods of two distinct modes of observation and investigation. In biochemistry, we do not speak of, or try to define, ‘biochemical substances’; all elements and compounds are considered to be ‘chemical’, though some have more biological interest and application than do others. Similarly, we might say that in psychosomatic medicine we ought not to distinguish among different diseases: all disorders of the body and of behaviour belong—if social custom so decrees—in the domain of medicine. (What constitutes a ‘disease’ is itself in part socially determined.) Among these, some disorders appear to be more, and others

¹ See in this connection S. Butler, *Erewhon* (1872), Penguin Book Edition.

² F. Alexander in F. Alexander, T. M. French, et al., *Studies in Psychosomatic Medicine*, New York, 1948, p. v

³ R. R. Grinker, *Psychosomatic Research*, New York, 1953, p. 14

less, interesting from a psychological point of view and the former syndromes have become the chief objects of study in 'psychosomatic medicine'. To invoke the patient's response to treatment in the definition of 'psychosomatic disorder' would only complicate matters in a field which, as Walker himself notes, already abounds in metaphysical obscurities.

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Note on Descartes and Psychosomatic Medicine

MR NIGEL WALKER has performed a valuable service in his survey of the possible connotations of the term 'psychosomatic'. In view of the fact that it should be widely read and form the basis of discussion of this important question, it is desirable that the further dissemination of the 'official' (Ryle's term) version of Descartes's treatment of the mind-body problem should be checked at the outset. This 'official' version is expressed by Ryle ('with deliberate abusiveness') as the dogma of the 'ghost in the machine'; and by Walker as the 'pilot in the cockpit'. Now when Descartes wrote 'I knew that I was a substance whose *whole essence or nature consists entirely in thinking*' and that '*the soul . . . is entirely distinct from the body*',¹ it cannot be denied that he was illicitly hypostasising two aspects of the unitary experience, 'consciousness of body'. Nor can it be claimed that he ever found any solution of the pseudo-problem thereby created, that is, the relation between the 'thinking substance' and the 'extended substance'. What can, however, be claimed is that the 'Cartesian dualism' of Walker and the 'ghost in the machine' of Ryle represent a shift which he *specifically* repudiated. In the *Discours* he wrote 'Il ne suffit pas qu'elle [sc. "ame raisonnable"] soit logée dans le cors humain, ainsi qu'un pilote en son navire, sinon peuestre pour mouvoir ses membres, mais qu'il est besoin qu'elle soit jointe & unie plus estroitement avec lui pour avoir, outre cela, des sentimens & des appetits semblables aux nostres, & ainsi composer un vrai homme'.² For 'ship' read 'cockpit' and you have Walker's 'Cartesian dualism'. It is clear that in this matter Descartes was not a 'Cartesian'. This repudiation is repeated even more strongly in the *Meditationes*, where the corresponding passage ends '*. . . adeo ut*

¹ Adam and Tannery, *Oeuvres*, Tome 6, 33 (italics mine)

² *ibid.* 6, 59

PSYCHOSOMATIC DISORDER

unum quid cum illo componam'.¹ As if anticipating Ryle's alternative 'escape route', in a letter to Regius he wrote: 'Si enim angelus corpori humano inesset, non sentiret ut nos, sed tantum perciperet motus qui causarentur ab objectis externis, et per hoc a vero homine distingueretur'.² Is there so much difference between a 'ghost' and an 'angel'?

This clearing of Descartes from the imputation of being responsible for either the 'official' or the 'Cartesian' dualism is not merely an act of piety or even a claim for historical accuracy. Its further purpose is to encourage 'second thoughts' in regard to the relevance of the writings of Descartes to the construction of a sound theoretical basis for modern psychosomatic medicine. For the passages just cited are not the only ones in which he suggests at least a plausible article of faith. Part of the Sixth Book of the *Discours* is taken up with his concern for giving 'life more abundantly'. Thus he says: 'For the mind is so dependent on the temper and disposition of the bodily organs that if any means can ever be found to render men wiser and more capable than they have hitherto been I believe that it is in the science of medicine that the means must be sought'. And it is clear from this and the 'unum quid', that the form this future medicine would take might not inaptly be described as 'psychosomatic'.

WILLIAM P. D. WIGHTMAN

Psychosomatic Disorder: a Rejoinder to Wightman and Szasz

As Wightman first introduced me to Descartes, he is doubly entitled to complain if I have misrepresented him; and I accept the correction. Wightman does not of course claim that there are no such people as Cartesian dualists (merely, as he neatly says, that Descartes was not one); and there is no doubt that in the field of physicians' metaphysics they have caused a great deal of confusion.

I have more difficulty in accepting Szasz's criticisms. If I understand them, they can be separated into the following objections:

(i) 'symptom' means 'what the patient complains of', and is thus always psychic, although in some cases the physician observes that the complaint is 'validated' by physiological 'signs'. This is surely carrying the physician's distinction between 'symptoms' and 'signs' too far. One might just as well argue that what we call psychic symptoms are really somatic because they are discovered through the physiological processes of speech or other bodily

¹ Adam and Tannery, *Oeuvres*, Tome 7, 81

² *ibid.* 3, 493. Quoted by F. Alquié, *La découverte métaphysique de l'homme chez Descartes*, Paris, 1950, p. 302.

behaviour. But I am quite willing to say that my 'somatic symptoms' can be translated into Szasz's language by the words 'physiological signs', and it might have been wiser if I had used this phrase.

(ii) '*Improvement*' in organic (sic) medicine is judged by the disappearance of symptoms, but in psychotherapy means '*working through*', etc. This objection is expressed in the very language which the first one attacks. But it has graver weaknesses. First, does Szasz really mean that psychotherapy is not to be judged by the disappearance of symptoms? It is true, as my paper pointed out, that psychotherapeutic treatment may substitute psychic for somatic symptoms; and that the conscientious psychotherapist would continue treatment until he had dealt with the latter. This is not, however, an objection to my definition but merely a consideration in deciding whether psychotherapy is advisable in the case in question. Secondly, the criteria for psychotherapeutic cure which Szasz *does* offer are really the technical devices by which the cure is supposed to be effected. To judge a patient's cure by his success in '*working through*' is like calling a malaria patient cured because he has tolerated his doses of quinine.

(iii) *By treating psychosomatic disorders as a distinguishable class I have tried to solve a pseudo-problem.* This objection misses the point of my paper, which was that physicians' attempts to fit these disorders into their systems of classification by symptom and cause had led to the creation of a metaphysically unsound category, and that the solution is to realise that these are simply disorders which raise the technical question 'Should psychotherapy be used for this case?', just as other disorders raise the question 'Shall we operate?'

NIGEL WALKER

Historical Determinism, Social Activism, and Predictions in the Social Sciences

IN a recent issue of this *Journal*, M. Roshwald maintained that it was *self-contradictory* for Marx to combine a belief in the need for exhorting men to establish socialism with the thesis of the historical inevitability of its establishment.¹ Presumably, Roshwald's criticism of this feature of Marx's theory does not depend on the merits of Marx's particular prognosis of the career of industrialism, but derives from Roshwald's view that it is inconsistent for *any* deterministic socio-political theory to *advocate* a social activism with the aim of thereby bringing about a future state whose eventuation the theory in question regards as assured by historical causation. Since

¹ This *Journal*, November 1955, 6, 191, n. 3. A similar contention is found in George L. Kline's 'A Philosophical Critique of Soviet Marxism', *The Review of Metaphysics*, 1955, 9, 100.

PREDICTIONS IN THE SOCIAL SCIENCES

Roshwald's thesis does not depend on whether the explanatory variables of the historical process are held to be economic, climatic, demographic, geopolitical, or the inscrutable will of God, it applies not only to the Marxian view but also, *mutatis mutandis*, to each of the following: (i) Augustine's (and Calvin's) belief in divine fore-ordination, when coupled with the advocacy of Christian virtue, (ii) the advocacy on the part of a British economist, who pays taxes in England, of the passage of a certain tax-law by Parliament for the purpose of assuring a specified income distribution, when combined with a deterministic *prediction* by him of the passage of that tax-law and of the ensuing desired income distribution, (iii) Justice Holmes' dictum that the inevitable comes to pass through effort.

In the present note, I wish to challenge (1) Roshwald's contention that historical determinism is logically incompatible with the advocacy of social activism; in addition, I shall deal critically with the following claims: (2) Roshwald's assertions that 'An ethics, . . . , combined with a strictly deterministic philosophy, would have no practical significance as a motivating force' and that determinism 'implies the practical futility of discussion about the best way to be chosen by men in forming their social relations',¹ (3) Robert Merton's contention that the 'self-stultifying' and 'self-fulfilling' predictions encountered in the social sciences are 'peculiar to human affairs' and are 'not found among predictions about the world of nature'.² The inclusion of a rebuttal of Merton's statement by means of counter-examples is prompted by the fact that his statement might otherwise be adduced in support of Roshwald's cardinal tenet that 'social science . . . seems as radically different from natural science as man is from inanimate objects'.³

(1) Although the predictions made by a contemporary (Marxian or anti-Marxian) historical determinist concerning the social organisation of industrial society and those made by our British economist pertain to a society of which these forecasters are members and are thus *self-referential*, they are made by social prophets who, *qua* deterministic forecasters, consider their own society *ab extra* rather than as active contributors to its destiny. But the predictions made from that theoretically external perspective are predicated on the prior fulfilment of certain initial conditions which include the presence in that society of men who are dissatisfied with the existing state of affairs and are therefore actively seeking the future realisation of the predicted social state. To ignore that the determinist rests his social prediction in part on the existence of the latter initial condition, just as much as a physicist makes a prediction of a thermal expansion conditional upon the presence of heat, is to commit the fallacy of equating determinism with

¹ Roshwald, *op. cit.*, p. 192

² Robert K. Merton, *Social Theory and Social Structure*, Glencoe, 1949, p. 122

³ Roshwald, *op. cit.* pp. 202-203

fatalism.¹ Now, in actual fact, the social forecasters whom we are considering are not only spectatorial theoretical analysts of the society whose future they are predicting but, *qua* members of that society, also participate in the fashioning of its destiny. On what grounds then does Roshwald feel entitled to maintain that it is logically inconsistent for them, *qua* participating citizens, to advocate that action be taken by their fellow-citizens to create the social system whose advent they are predicting on the basis of their theory? Is it not plain now that Roshwald's charge derives its semblance of plausibility from his confusion of determinism with fatalism in the context of self-referential predictions?

(2) Roshwald's denial of the practical significance of an ethics which is coupled with determinism is tantamount to asserting that a belief in determinism is *causally* incompatible with the determinist's possession of the psychological incentives necessary for taking the action required to implement the moral directives of his ethics. This contention is both empirically false and inconsistent with Roshwald's own premisses. For Abraham Lincoln's view that his own beliefs (ethical and other) were causally determined did not weaken in the least his desire to abolish Negro slavery, as demanded by his ethical theory, and similarly for Augustine, Calvin, Spinoza and a host of lesser men. Moreover, Roshwald's mention of 'practical significance as a motivating force' shows that he is actually affirming a *causal* connection between two kinds of psychic states: a belief in determinism and indolent utilitarianism. But this thesis not only contradicts his correct observation that '*psychologically* the necessity of the "final victory" moved the adherents of the [Marxist] creed to participate in its realisation and served [as] a powerful source of revolutionary activity'² but constitutes a covert and unwitting invocation of psychological determinism to which Roshwald himself is hardly entitled.

Equally untenable is his claim that it is practically futile for determinists to weigh alternative modes of social organisation with a view to optimising their own social arrangements. For the determinist does *not* maintain, in fatalist fashion, that the future state of society is independent of the decisions which men make in response to (i) facts (both physical and social), (ii) their own *interpretation* of these facts (which, of course, is often false), and (iii) their value-objectives. It is precisely because, on the deterministic theory, human decisions *are* causally dependent upon these factors that deliberation concerning optimal courses of action and social arrangements can be reasonably expected to issue in successful action rather than lose its

¹ For a discussion of this fallacy and of related issues, see A. Grünbaum, 'Causality and the Science of Human Behavior', *American Scientist*, 1952, 40, 671 [reprinted in Feigl and Brodbeck (eds.), *Readings in the Philosophy of Science*, New York, 1953, pp. 766-778] and 'Time and Entropy', *American Scientist*, 1955, 43, 568-570.

² Roshwald, *op. cit.*, pp. 191-192, n. 3

PREDICTIONS IN THE SOCIAL SCIENCES

significance by adventitiousness. Roshwald's objection here springs from the false supposition that if our beliefs and decisions have causes, these causes force the beliefs in question upon us, against our better judgment, as it were, thus rendering the attempt to exercise that judgment futile.

(3) It remains to consider Robert Merton's interpretation of what John Venn has called 'suicidal prophecies' in the social sciences. As an example of such a prophecy, Merton cites a government economist's distant forecast of an oversupply of wheat which, upon becoming publicly known, induces individual wheat growers so to curtail their initially planned production as to invalidate the economist's forecast.¹ Since failure to take cognisance of the possible 'perturbational' influence of the dissemination of a social forecast may issue in that prediction's *spurious disconfirmation*, Merton rightly points out that the possibility that a social prophecy stultify itself by becoming known creates a problem for the reliable testing of social predictions. It would, of course, be an error to infer that the phenomenon of suicidal prophecies encountered in the social studies constitutes evidence against determinism, since the dissemination of these prophecies alters the initial conditions on which the forecasts were predicated and would not prevent another forecaster, at least in principle, from correctly predicting the outcome on the basis of the actual, modified initial conditions.² Merton does not commit *this* error. But he does make the equally vulnerable claim that self-stultifying and self-fulfilling prophecies are endemic to the domain of human affairs and are 'not found among predictions about the world of nature'. As evidence, he cites the fact that a 'meteorologist's prediction of continued rainfall has until now not perversely led to the occurrence of a drought' and that 'predictions of the return of Halley's comet do not influence its orbit'.³ To be sure, these particular predictions of purely physical phenomena are not self-stultifying any more than those social predictions whose success is essentially independent of whether they are made public or not. But instead of confining ourselves to commonplace meteorological and astronomical phenomena, consider the goal-directed behaviour of a servo-mechanism like a homing device which employs feedback and is subject to automatic fire control. Clearly every phase of the

¹ Merton, *op. cit.*, p. 122

² The dissemination of the *adjusted* prediction may then, in turn, require that the *latter* forecast be revised to allow for the effects of its publication, and so on *ad infinitum*. Fortunately, successful prediction is possible nonetheless in such cases under very general conditions, as has recently been shown by E. Grunberg and F. Modigliani in an article 'The Predictability of Social Events' [*The Journal of Political Economy* (U.S.A.), 1954, 62, 465] to which Professor H. Feigl has kindly called my attention. These authors elaborate their thesis by reference to the public prediction at time t of the price of some commodity that will prevail at time $t + 1$ and then generalise this analysis of the one-variable case to cover the correct public prediction of the values of n variables.

³ *ibid.*, p. 181

operation of such a device constitutes an exemplification of one or more purely *physical* principles. Yet the following situation is *allowed* by these very principles: a computer predicts that, in its present course, the missile will miss its target, and the communication of this information to the missile in the form of a new set of instructions induces it to alter its course and thereby to reach its target, contrary to the computer's original prediction. How does this differ, in principle, from the case in which the government economist's forecast of an oversupply of wheat has the effect of instructing the wheat growers to alter their original planting intentions?

Cotresponding remarks can be made concerning self-fulfilling prophecies, which Merton likewise believes to be confined to the realm of human affairs. Such prophecies are characterised by the fact that an *initially false* rumour is believed and thereby creates the conditions of its own fulfilment and spurious confirmation: when the depositors of a bank whose assets are initially entirely adequate give credence to a false rumour of insolvency, this belief can inspire a run on the bank with resulting actual insolvency. Merton shows illuminatingly how the pattern characteristic of self-fulfilling predictions serves to buttress prejudices against minority groups by the mechanism of spurious confirmation.¹

Is there any analogue to such predictions in the domain of physical phenomena? It is at least physically possible, though not very likely, that the reception of a false message (e.g. a false warning of an impending collision) will actuate a servo-mechanism so as to realise the very conditions which the originally false message predicted.

It would be unavailing to object that the counter-examples which I have adduced against Merton's contention involve physical artifacts which depend for their existence upon having been constructed by human beings. For the phenomena in question fall entirely within the purview of physical laws and thus invalidate the widely-held belief, espoused by Merton, that, in principle, self-stultifying and self-fulfilling prophecies are 'not found among predictions about the world of nature'.

ADOLF GRÜNBAUM

The Case for Indeterminism

IN his reaction to my paper on 'Value-Judgments in the Social Sciences', Professor Grünbaum tackles the ages-old issue of determinism in human affairs, which was merely cursorily hinted at by me, as it affected value-judgments in the social sciences. The problem itself is too fundamental and important, too ancient and modern, to be dismissed in a short discussion.²

¹ Merton, *op. cit.*, pp. 179-182

² An excellent recent analysis of the issue can be found in an essay of Isaiah Berlin, *Historical Inevitability*, Oxford, 1954.

THE CASE FOR INDETERMINISM

However, a few remarks may help a little to clarify this issue. These will be limited to what seems to me the central point in Grünbaum's approach rather than following the details of his criticism. The answer on the details is implied—I believe—in my reply.

I maintained with reference to Marx—but Grünbaum rightly understands that the thesis is of a *general* character—that 'historical determinism is logically incompatible with the advocacy of social activism'. If the final victory of the proletariat is *inevitable* and *necessary*, why on any logical reason should one take the trouble of helping history to run its inevitable course? To this Grünbaum answers that 'the predictions made from that theoretically external perspective are predicated on the prior fulfilment of certain initial conditions which include the presence in that society of men who are dissatisfied with the existing state of affairs and are therefore actively seeking the future realisation of the predicted social state'. If I understand this statement right, it amounts to saying that historical prognosis holds true provided certain human conditions are present. In other words, if for some reason some men in that society become *satisfied* with the existing state of affairs, the prediction of the theorist may become false!

This is certainly less than Marxist determinism, if it is determinism at all. Anybody can construct prognostic social theories at his pleasure, if such conditional clauses are allowed. Here are a few examples: 'Provided human beings remain irrational, the world will progress towards inevitable destruction.' 'Provided the French behave like the English, their internal political difficulties will diminish.'

The issue is not mainly one of the theory, but one of the conditioning clause. If Grünbaum contends that there are certain causal connections in social and human issues, nobody would be so bold as to deny it. But our social activity consists of influencing those causal factors and thus modifying the course of society and history. This social activity itself may again be causally explained: one's action may be linked with upbringing, genetic factors, social environment, economic needs, and also with *correct reasoning* and an *alert conscience*. If all this is allowed for, indeterminism is saved. For if *indeterminism* in respect of human issues has any meaning at all, it is the admittance of the significance of the *determining* factors of reason and conscience.¹ If anybody prefers to call this determinism instead of indeterminism, I would not enter on a dispute which would be purely verbal.

¹ It can be argued that if all human beings were governed *solely* by right reason and conscience, the course of history would be easily predictable. As things are today, however, we are very far from such a situation. Moreover, the protagonists of determinism hardly ever think of such a possibility and try to found their theories on *non-voluntary* factors, regarding independent reasoning as an (illusory) bastion of indeterminism. Therefore, in the usual sense, allowance for individual reasoning signifies an allowance for an indeterministic element.

If, however, the meaning of Grünbaum's determinism is that there exists a *fixed pattern* of causal factors determining human behaviour, and it is only up to the social scientist to discover it, then I do not see how this should differ, in substance, from what he derogatorily calls 'fatalism'. 'The fatalist says that regardless of what we do, the outcome will be the same.'¹ The reason for this belief of the fatalist may be some sort of animism, or the belief in an omniscient and omnipotent God, or any other factor not admitted by natural sciences. However, to say (in the spirit of determinism) that whatever happens is strictly pre-determined by an extremely complicated pattern of factors—economic, physiological, psychological, and so forth—is fatalistic as well, in as much as it supposes a *necessary* outcome. The only difference between fatalism and determinism would be in the *nature* of the causal pattern affecting social (or natural) events, but otherwise determinism is as fatalistic as fatalism, unless it allows for a certain indeterministic gap.

It might be maintained that the determinism in the social phenomena is only 'statistical'² and that there is enough uniformity in the pattern of causal factors to assume 80 per cent accuracy in predictions of the social scientists. However, we contend that it is doubtful whether any predictions of the social scientists are valid even statistically, except if they are limited in *area* and *time*. A social scientist may predict perhaps with fair expectation of accuracy how many television sets will be sold in New York during the next year. He cannot predict how many will be sold throughout the world during the next century. The reason for this limitation is not only the *complexity* of the factors determining this phenomenon, but also in the immensurability of the factor of *human reflection*. The percentage of unbiased thinking human beings in a certain commercial area may be today 1 or 2, and the statistical prediction based on previous successes of high-pressure salesmanship fairly accurate. In a few decades, however, perhaps with the help of a less deterministically-minded education, the numbers of such people may increase. The degree of actual significance in human behaviour of the factor of rational judgment seems by no means fixed, and it may radically change from generation to generation. The irrationality of the present generation no more guarantees the irrationality of the future generations than the rationality of some eighteenth-century thinkers did guarantee the rationality of our era. Here is a variable which shrinks or extends, and is bound to perplex those students of man and society whose ambition is to be called *scientists* in the sense of the natural scientists.

M. ROSHWALD

¹ A. Grünbaum, 'Causality and the Science of Human Behaviour', *American Scientist*, 1952, 40, 671

² *ibid.*, pp. 670-671

CONTENT AND DEGREE OF CONFIRMATION

Remarks on Popper's Note on Content and Degree of Confirmation

IN a recent note,¹ Popper has raised objections against some points in my book on probability² and a later paper of mine.³ I shall not enter into a substantial discussion of Popper's arguments but merely correct some points where he attributes to me statements which I did not make.

(a) Popper mentions (p. 158) the three concepts of confirmation which I distinguish, viz. (i) the classificatory, (ii) the comparative, and (iii) the quantitative concepts of confirmation. Referring to my book, he says (p. 158): 'All three concepts are discussed at some length; but in the end, only a theory of (iii) is offered', and later (p. 158 n.): 'Thus no current theory of either the classificatory or the comparative concepts is claimed to exist.' These statements are not correct. In fact, I gave a definition for (i), based on the degree of confirmation c (*Probability*, p. 463, (2)). According to this definition, (i) is the same as the concept of positive relevance. An analogous definition for (ii) based on c is obvious (see *Comparative*, p. 311, (3)); thus the theory of (ii) is part of the theory of c given in *Probability*, Chapter IV (see the list on p. 456 of quantitative theorems with a merely comparative content). Popper supports his assertions by the following alleged quotations from *Probability*:

- (1) 'This concludes the discussion of the classificatory concept. We have not found an adequate explicatum . . .';

and on the comparative concept:

(2) 'However . . . it seems doubtful whether a simple definition can be found.' If I had made these statements as quoted, the reader might indeed conclude that I did not see any way of defining either (i) or (ii), although he might wonder why I had forgotten so soon my own definition of (i). What I actually said was quite different. Instead of (1), the book says:

- (1') 'This concludes the discussion of the classificatory concept of confirming evidence. We have not found an adequate explicatum in *non-quantitative terms*' (p. 482).

Then it continues as follows:

- (1'') 'The concepts which were considered as possible explicata were found to be too narrow. *However, we have a theory of confirming evidence* [i.e. concept (i)] *in quantitative terms*. The general part of this theory . . . was constructed in the preceding chapter as the theory of relevance.'

Instead of (2), the book says:

- (2') 'However . . . it seems doubtful whether a simple definition in *L-terms* can be found' (p. 467).

(The italics in (1'), (1''), and (2') are not in the original.) The essential point of my discussion of the concepts (i) and (ii) in the book was the following. The obvious

¹ Karl R. Popper, '"Content" and "Degree of Confirmation": A Reply to Dr Bar-Hillel', this *Journal*, 1955, 6, 157-163

² R. Carnap, *Logical Foundations of Probability*, 1950, here referred to as *Probability*

³ R. Carnap, 'On the Comparative Concept of Confirmation', this *Journal*, 1953, 3, 311-318, here referred to as *Comparative*

definitions of these concepts use quantitative terms, viz. the degree of confirmation, while I tried, without success, to find definitions using only non-quantitative terms, e.g. *L*-terms. In each quotation Popper omits a few words which would have destroyed his argumentation; and the sentence in (1'') here italicised directly contradicts his assertion. (The discovery of the omission in (1) is not made easier by the fact that Popper refers to page 492 instead of page 482. The omission in (2) is not even indicated by dots.)

(b) Popper reports correctly (p. 158) that my definition of degree of confirmation (viz. c^* , *Probability*, § 110) leads to the result that the degree of confirmation for any universal law for a universe with infinitely many individuals is zero. Like many others, he regards this result as counter-intuitive. This is a serious problem, which I shall not discuss here. However, Popper continues (p. 159): 'And Carnap himself admits that this result is counter-intuitive.' This assertion, which he repeats again later, is not correct. On the contrary, my whole discussion (in § 110 G) tries to show that the result, in spite of the first appearance, is not counter-intuitive. I can easily imagine that a reader might remain unconvinced by my arguments. But it is thoroughly puzzling to me how any reader could have the impression that I myself believed the proposition which I tried so hard to refute.

(c) Popper shows correctly (p. 160) that the following theorem holds for logical probability p :

(3) If x follows from y , then, for every z , $p(y, z) \leq p(x, z)$.

He adds: 'which is, precisely, the invalid condition which Carnap uses on the bottom of page 474 of *Probability* as an argument to show the invalidity of a confirmation concept'. This is an error. If I had actually asserted the invalidity of (3) or rather of its analogue for degree of confirmation, as Popper thinks, then my theory would indeed contain a glaring inconsistency; for I myself have asserted this analogue as a theorem (T59-2d, p. 317). The condition, which I showed to be invalid and used as stated by Popper (I call it 'special consequence condition', as Popper mentions correctly), is in fact the following (in the simple form for initial confirmation, see *Probability*, p. 471 (H 8.21) and p. 464 (4), ' t ' is the tautology):

(4) If x follows from y , then, for every z ,

if $c(y, z) > c(y, t)$ then $c(x, z) > c(x, t)$.

The conditions (3) and (4) have a certain similarity but are not the same. I have explained their difference in *Probability* page 475.

Popper's whole argument (p. 160) is based on the confusion of (3) with (4) and collapses with it.

RUDOLF CARNAP

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Reply to Professor Carnap

PROFESSOR POPPER writes: In section (a) of Professor Carnap's paper I am charged with misquoting him. I am sorry that I failed to insert three dots in order to indicate an omission in quotation (2), and that I referred to page 492 instead of page 482. I admit

PROBABILITY AND CONFIRMATION

these two mistakes and I apologise. But fortunately these regrettable errors do not affect the argument at all. Nor do they give grounds for Professor Carnap's complaint. This complaint is directed not against an argument of mine but against a mere *footnote* of eight lines (and he gives the reader no warning that the two mistakes both occurred in that footnote). The purpose of the footnote was to help the reader to find passages in *Probability* which support my contention: the contention that it is sufficient for me to discuss (iii), i.e. the 'quantitative' concept of confirmation, since no theory ('independent of (iii)', I ought to have added) of the other two concepts is offered. The purpose of my footnote was, no doubt, partly defeated by printing '492' instead of '482', and to a lesser extent by omitting the three dots. These two mistakes of mine are incontestable *facts*. All the rest is Professor Carnap's *interpretation*. The suggestion that I propped up my criticism by 'alleged quotations' is absurd. (Readers of the more 'substantial discussion' between Dr Bar-Hillel and myself will be able to judge whether my arguments are in need of such props to prevent them from collapsing.)

My reply to Professor Carnap's section (b) will be found, in this number, in sections (5) and (6) of my 'Adequacy and Consistency: A Second Reply to Dr Bar-Hillel'.

My reply to Professor Carnap's section (c) is this. Professor Carnap asserts that I confuse two formulae which he numbers (3) and (4), and he says that (3) is valid while (4) is false. But as I show, in section (8) of my 'Second Reply', (4) follows from (3). Thus Professor Carnap's assertion is logically inconsistent; and he is seriously in error if he believes that my argument collapses because of the alleged 'confusion' of (3) and (4).

K. R. POPPER

Further Comments on Probability and Confirmation

A Rejoinder to Professor Popper

(1) PROFESSOR K. R. POPPER, in his reply to my comments on a note of his, does not accept my view that the disagreement between him and Professor R. Carnap on questions of logical probability and degree of confirmation is mostly a verbal one. On the contrary, in this reply he goes on to make much stronger claims than he did in the first note and charges that Carnap's theory of confirmation, as presented in his two recent books, 'is partly inconsistent, and partly inadequate from the point of view of *his own* requirements, not merely from that of *my* (Popper's) requirements' (*Reply*, p. 158).

Because of the severity of these charges and the great importance of the issues behind them—I think it is no exaggeration to state that the problems around the logic and methodology of induction occupy the central position in modern philosophy of science—and because of the fact that Carnap's works on inductive logic are not so well known among British logicians and methodologists as they deserve to be, in my opinion, it might be worth-while to dedicate more space to this discussion than I did in my very brief *Comments*.

Carnap himself will answer Popper's *Reply* in so far as it is based upon attributing to Carnap statements which he did not make. This will enable me to restrict myself

to the substantive side of the issue. I shall, however, rely occasionally on Carnap's remarks.

(2) It should by now be perfectly clear that the terms 'logical probability', 'degree of confirmation', 'degree (measure) of relevance', 'degree (measure) of (evidential, factual) support' (and others), in their pre-systematic uses, cover at least two explicanda which, strongly related as they may be, are still quite different. (This ambiguity is, of course, different from that of the term 'probability' itself, exhibited and treated by Carnap as 'probability₁' vs. 'probability₂'.) A hypothesis h with a high initial (or absolute) logical probability will retain the same high degree of probability relative to any evidence-statement e that is irrelevant to it (from which h is independent). h may even retain a high probability, though not as high as before, in face of an evidence-statement e that is negatively relevant to it (that undermines it). All this is commonplace and probably would not cause any controversy, were it not for the fact that Carnap would, in his systematic use of 'degree of confirmation' (but also in some pre-systematic uses of this term), express the situation by saying that in both cases h has a high degree of confirmation on e , which can easily be paraphrased by saying that h is highly confirmed by e , which sounds paradoxical enough since e is either irrelevant to h , in the first case, or even negatively relevant, in the second case.

(3) Had Popper only called attention to this terminological oddity and insisted on having it revised, reserving the expression 'degree of confirmation' for a pre-systematic synonym (or systematic explicatum) of 'degree of relevance', he would have a strong point, and I, for one, would have had no objection. It even seems to be the case that Popper himself did use this term, or rather its German equivalent '*Grad der Bewährung*', in this sense and that Carnap did not notice immediately that he was deviating from this use when he began using the term himself somewhat later, thereby adding to the confusion. And it is true that Carnap, as late as in 1950, in section 41A of *Probability*, presented one of the characterisations of probability₁ (logical probability) in terms of strength of support, which is indeed definitely misleading. It seems that by stating there, 'To say that the probability₁ of h on e is high means that e gives strong support to the assumption of h , that h is highly confirmed by e . . .' (p. 164), Carnap himself fell prey, for a moment, to the ambiguity of the pre-systematic usage of 'highly confirmed', and that he (erroneously) identified 'the probability of h on e is high' with ' e gives strong support to h ', in this preliminary discussion, just because both locutions can somehow be replaced by 'the degree of confirmation of h on e is high', the second in ordinary language, the first in Carnap's own technical sense.

(4) Popper, however, was not satisfied by calling attention to these inadvertencies but went on to charge Carnap's systematic use of 'degree of confirmation' with inconsistency and inadequacy, as mentioned before. I intend to show that the charge of inconsistency is completely unfounded, whereas the charge of inadequacy is justified only to a very limited degree.

(5) Popper is aware of the difficulties involved in showing that a certain explication is inadequate, in view of the vagueness of the formulations in which the conditions of adequacy are generally, and necessarily, couched. He still believes that in our case the divergence from one of these conditions is so flagrant as to leave no doubt about the inadequacy. The violated condition is that of sufficient agreement with intuition, where 'sufficient' is to be interpreted as 'approximate'. The inadequacy

PROBABILITY AND CONFIRMATION

consists then in the fact that the initial as well as any relative value assigned by Carnap's c -functions to a universal hypothesis in a universe with infinitely many individuals is zero, whereas ordinary intuition would regard at least some of these hypotheses as highly confirmed, and hence insist upon having assigned to them a high degree of confirmation.

If Popper is right, then he has indeed shown that Carnap's c -functions are inadequate explicata. This, of course, would not at all deprive these functions of all value. There are innumerable scientific concepts that can by no means be regarded as explicata of pre-scientific notions and are nevertheless of highest importance. But Carnap's work would certainly lose much of its philosophical significance.

(6) But is Popper right? Is a value zero for a would-be explicatum of a logical-probability function for a general law in an infinite universe really counter-intuitive? The gist of Carnap's argument in *Probability*, section 110G, is that this is not so, at least not for *guided* intuition, though it might be so for unguided intuition. Carnap believes that he is in a position to persuade those who, for intuitive reasons, would like to assign to a 'well-confirmed' law a high value of logical probability not to insist on this requirement, in case it should lead to technical difficulties, and to look on this inclination as a rather misguided expression of their desire to assign high probability-values to the next few instances of the law. There are some good arguments that can be brought forward in support of Carnap's attitude, and he will doubtless exhibit them in the second volume of *Probability*. I am not sure that they will convince Popper. But even if they did, it must be admitted that the qualification 'guided' that has to be prefixed to 'intuition' in the phrase 'sufficient agreement with intuition' is a major change in the formulation of the adequacy conditions, and a change to the better, to my mind.

(7) Further, Popper has separate objections against Carnap's notions of instance-confirmation and qualified instance confirmation. I did not understand his first reason for the claim of inadequacy of the unqualified instance confirmation (*Reply*, p. 160). His second reason consists in asserting that Carnap's definition of this notion as well as that of the qualified instance confirmation is inconsistent, because it is hit by the paradox of confirmation (*Reply*, p. 161). I glanced, at Popper's invitation, at the two pages of *Probability* (p. 572 and p. 469) that were supposed to contain the inconsistency but could find none. Sticking to Carnap's definition for ' c_{qi} ', given in *Probability*, page 573, it is rather obvious that the values of c_{qi} will be invariant with respect to any replacement of one of its arguments by an L -equivalent one. What now made Popper think differently? He seems to have been misled by the fact that the qualified instance confirmation of a law l_1 need not be the same as the qualified instance confirmation of a law l_2 which is L -equivalent to l_1 . The point is, of course, that l_1 and l_2 are not at all arguments of the relevant function (c_{qi}). (It is true that Carnap did not mention this point explicitly, but then he promised to deal more extensively with the whole topic in vol. II of *Probability*.¹)

¹ Popper suggests a 'rectification' of Carnap's definition of the qualified instance confirmation. He does not, however, say explicitly what he means to take as the definiendum. If he intends to retain Carnap's own definiendum, then his suggestion is an unnecessary complication, since Carnap's simpler form also fulfils the invariance requirement. However, if he intends to have ' $c_{qi}(l, e)$ ' as the definiendum, it can be shown that Popper's definition is hit by the very paradox it was aimed to avoid.

What has to be understood is that the value of the qualified instance confirmation of a law l having the form of a general implication, on a certain evidence e , is *not* the value of the degree of confirmation of l (on some evidence e') but rather the degree of confirmation of a new instance of the consequent of l on an evidence which is the conjunction of e and the corresponding instance of the antecedent of l .

The same misunderstanding made Popper claim somewhat further (*Reply*, p. 161) that Carnap's two concepts of instance confirmation led to absurd consequences. The first illustration presented by Popper for this purpose consists of a universe of coin tosses, with only two predicates 'heads up' and 'tails up'. Let the evidence e be 'out of twenty past tosses ten were heads up and ten tails up', let the hypothesis h be 'all future tosses will be heads up'. Under these circumstances, charges Popper, everybody would agree that the hypothesis h has been amply refuted by the evidence, whereas Carnap assigns to this hypothesis on this evidence an unqualified instance confirmation of $\frac{1}{2}$. The truth is, of course, that Carnap too assigns to h a very low confirmation value—for an infinite universe just zero—whereas the confirmation value $\frac{1}{2}$ is assigned not to h —and this in spite of the English formulation—but rather to an instance of h . And this value looks very reasonable, since it seems to be fair to bet on this evidence with even odds that the next toss will result in heads up. (Popper does never mention this characterisation of degree of confirmation, i.e. as a fair betting quotient, though Carnap himself regards it clearly as a more adequate characterisation than that through evidential support.)

(8) The seemingly strongest objection of Popper's, viz. that connected with the *content-condition* and discussed in *Reply*, page 160, is based upon a simple confusion, as Carnap shows in his *Remarks*, and will therefore not be discussed here.

(9) In conclusion: Though Popper's criticism of Carnap's position seems again to be objectively unfounded, in the main, and based upon factual errors as well as misunderstandings, it forcefully shows the necessity of further discussion of the exact relationships between the systematic uses of such terms as Carnap's 'degree of confirmation', 'relevance measure', 'instance confirmation', Popper's 'logical probability', 'degree of confirmation' or Kemeny-Oppenheim's 'degree of factual support' and the pre-systematic uses of these terms as well as of 'measure of evidential support', 'content', 'acceptability' and 'reliability'.¹ It is indeed Carnap's contention that all these pre-systematic usages—in their semantic aspects—can be explicated in terms of (regular) c -functions (though not, of course, always as c -functions). One of Popper's main aims in his recent series of notes seems to have been to challenge this contention. I believe that he failed to substantiate his objections. He did, however, succeed in calling attention to some weaknesses in current terminology, and it might perhaps be advisable to change these formulations accordingly, in addition to giving new thought to the clarification of the relationship between systematic and pre-systematic usages of the current terms in the field of probability and confirmation.

Y. BAR-HILLEL

¹ I do not believe that Carnap would want to regard either his degree of confirmation or his relevance measure as explicata of the *acceptability* of a theory, though he might have done so in the past. He does, however, regard instance confirmation as a measure of the *reliability* of a law. Cf. *Probability*, p. 572 and *Reply*, p. 162 and n. 2.

ADEQUACY AND CONSISTENCY

Adequacy and Consistency : A Second Reply to Dr Bar-Hillel

(1) IN his first 'Comments' on my note 'Degree of Confirmation', Dr Bar-Hillel asserted (a) that my criticism of what he called the 'current theory' raised *merely verbal issues*, and (b) that my 'point', so far as I had one, had been *fully anticipated* by the 'current theory'.

In his 'Further Comments', a similar line is taken. Again it is asserted that my criticism is merely verbal; 'objectively' (*sic*) unfounded; and merely showing that the 'uses of . . . terms' is in need 'of further discussion'. Yet as I will show, Dr Bar-Hillel admits at the same time all my 'points' (except one); and he withdraws, although only implicitly, all his earlier objections.

Before showing this in detail, I must, however, stress again that my views and the 'current theory' differ in fundamentals. I am interested in the main in the rôle of universal laws or theories. What I have tried to do was to explain why Einstein's theory of gravity is better confirmed or corroborated or more acceptable than Newton's. The 'current theory' of confirmation, on the other hand, implies that in a world like ours (which is very large, and perhaps even infinite) a zero degree of confirmation must be ascribed to all universal theories. (As to its 'instance confirmation' see point (7), below.)

Thus *scientific theories* which I consider as fundamental and indispensable for science, are at best 'expedients' from the point of view of the 'current theory'. Or in Professor Carnap's words: 'We see that the use of laws is not indispensable for making predictions. Nevertheless it is expedient, of course, to state universal laws in books on physics, biology, psychology, etc.'¹

I consider the gulf between this view (which, by the way, entails that prediction is the only, or the main, task of science) and my own view as unbridgeable. It is here where all those differences of opinion originate to which I now turn.

(2) I begin with my main point—the only point of my note, 'Degree of Confirmation', in which I criticised Professor Carnap. It was a footnote of ten lines.² My

¹ See R. Carnap, *Probability*, p. 575. The utter difference between my views and those here described is treated at length in my paper 'Three Views Concerning Human Knowledge', in *Contemporary British Philosophy*, 3 (edited by H. D. Lewis), 1956. See also my forthcoming paper 'The Demarcation Between Science and Metaphysics' which I contributed in January 1955 to the planned Carnap volume of the *Library of Living Philosophies*, ed. by P. A. Schilpp, and in which I try to show that Professor Carnap's various concepts of meaning (unintentionally) render scientific theories meaningless (and theology meaningful).

² Footnote 1 on p. 144. None of my other critical remarks in my 'Degree of Confirmation' refer specifically to Professor Carnap: they refer to the whole school of Keynes, Jeffreys, Broad, Russell, and many others. It is perhaps understandable that the defenders of the 'current theory' got the main points of my note 'Degree of Confirmation' quite out of focus, believing that 'The principal thesis of this paper is that Carnap's . . . degree of confirmation [is] . . . not suited to measure degrees of confirmation' (J. Kemeny, in *Journal Symb. Logic*, 1955, 20, 304). But 'The principal thesis' of my note is certainly not contained in its footnote 1, on page 144; rather is it a development of views which I first published more than twenty years ago.

point was this. Professor Carnap's distinction of two probabilities, logical probability (p_1) and statistical probability (p_2), is insufficient. There are, at the very least, *three* concepts to be distinguished, the third being degree of confirmation, or corroboration, or acceptability, which—as will be shown here again, in section (8)—*does not satisfy the formal laws of the probability calculus*.

I was surprised, when I first read Carnap's book, that he identified the first and the third of these concepts as a matter of course, without even hinting that this identification had been challenged and criticised by me so many years ago.

This was, and still is, my main point as far as my criticism of the 'current theory' is concerned. The point is now admitted by Dr Bar-Hillel under (2) when he writes (*italics mine*):

'It should by now be perfectly clear that the terms "logical probability", "degree of confirmation", [etc.] . . . cover at least *two* explicanda [i.e. *two* as yet not fully analysed ideas which it is our task to make precise] which . . . are *still quite different*.'

And later: 'Carnap himself fell prey, for a moment, to the ambiguity. . . .'¹

(3) With this, my main point—the one which has given rise to the controversy—is settled. It is, however, necessary to comment upon Dr Bar-Hillel's assertion, repeated several times—see his (1) and (3)—that my point was a *merely verbal* one, in opposition to what I indicated in my 'Degree of Confirmation' and what I said in my first reply.

But this assertion of Dr Bar-Hillel's is inconsistent with his own paragraph (2), 'It should by now be perfectly clear . . .'; for in (2), he admits that the issue is not verbal but that there are at least two different 'explicanda'—a term which, in the 'current terminology', means two ideas whose distinction is *not a merely verbal one* but presents a serious task to the 'logical analyst'.

Moreover, in his first 'Comments', Dr Bar-Hillel tried to establish the merely verbal character of my criticism by proposing a *Dictionary*. I showed, in my reply, that the proposed *Dictionary* was mistaken. Dr Bar-Hillel now drops it, silently; but a somewhat indirect admission that the *Dictionary* has been dropped may be found in the last paragraph of his 'Further Comments', when he writes (*italics mine*):

'In conclusion . . . Popper's criticism . . . forcefully shows *the necessity of further* discussion of the exact relationship between . . . such terms'; and he then lists some of the terms which he previously claimed, in his *Dictionary*, to be synonymous.

(4) Here our argument might have terminated. But Dr Bar-Hillel goes on, under (4), to contest my claim that the 'current theory' is inconsistent as well as inadequate.

¹ I myself happened to distinguish logical probability not only from degree of confirmation, or corroboration, or acceptability, but also from degree of support (' y supports (or undermines) x ') which is well represented by Kemeny and Oppenheim, 'Degree of Factual Support', *Philos. of Science*, 1952, 19, 307-324. Professor Kemeny (*Journ. Symb. Logic*, 1955, 20, 304) also recognises that there are (at least) *two* ideas, but he writes as if I had not seen this point which, he says, 'Carnap would be the first to admit'. But Professor Carnap has not admitted the difference even 'as late as in 1950'—as Dr Bar-Hillel now admits—or even now, as will become clear from my section (8): he '*pairs*' probability, the one idea, with (ungraded) confirmation, which is the other; and he still does not notice the clash with my (t). Nor does Professor Kemeny.

ADEQUACY AND CONSISTENCY

(5) He first discusses, under (5),¹ my contention that Professor Carnap's result—according to which the degree of confirmation of even the best confirmed universal law is zero (or approximately zero in a very large though finite universe)—is counter intuitive, according to Professor Carnap's own analysis (in *Probability*, § 110, G, first paragraph). This, I asserted, was fatal in view of the fact that the only criterion of adequacy discussed by Professor Carnap (*Probability*, p. 232) was agreement with an intuitive estimate of the degree of confirmation.

(6) Now even here we find an admission. Towards the end of (6),² Dr Bar-Hillel admits that, if we wish to rescue the current theory, we must replace the word 'intuition' by the new term, 'guided intuition': 'intuition' means, as he admits, what we should say, or think, *before* being indoctrinated by the current theory; 'guided intuition' means what we *may* say, or think, after having been indoctrinated. (He is not certain whether the indoctrination will work with people such as Popper.)

Comment is nearly superfluous: if intuition is to be used as a criterion of adequacy it can only be in its 'unguided' form.

It will now be seen that Dr Bar-Hillel's distinction between 'intuition' and 'guided intuition' which he himself rightly describes as 'a major change in the formulation of the adequacy condition' contains a full answer to section (b) of Professor Carnap's 'Remarks': I quoted the bottom of his page 571 and the top of page 572 in order to show that he himself pointed out that intuition (in what is now called its 'unguided' form) clashed with the ascription of a zero degree of confirmation (*not* of probability!) to all universal laws, however well confirmed. (What Professor Carnap now terms 'first appearance' is precisely the same as what Dr Bar-Hillel now terms 'unguided intuition' and what I would be inclined to describe as 'intuition before indoctrination'.)

(7) When Dr Bar-Hillel discusses my 'second reason' for claiming the inadequacy of the definition of instance confirmation, he refers to my assertion that 'this notion' (i.e. that of the *unqualified* instance confirmation) 'as well as that of the qualified instance confirmation is inconsistent, because it is hit by the paradox of confirmation'. This must be a slip of Dr Bar-Hillel's pen: a glance at page 161 of my reply will show him that under (2) I asserted *only* of the 'qualified instance confirmation' that it is hit by the paradox of confirmation. I wrote:

'(2) The qualified instance confirmation of which Carnap says in *Probability*, page 572, that it "seems in many cases to represent still more accurately what is vaguely meant by the reliability of a law *l*" is, I am sorry to say, inconsistent: it is

¹ The beginning of Dr Bar-Hillel's (5) may suggest that I accept intuition as a criterion of adequacy. This is not so: in my view, intuition is important but only as a stimulus which helps to create problems and to suggest possible solutions. I should never accept it as a judge, or as a court of appeal; for this would be the end of rational argument.

² In the beginning of (6), Dr Bar-Hillel seems to forget his admission that 'it should by now be perfectly clear that' we must distinguish logical probability from degree of confirmation. For he asks: 'But is Popper right? Is a value zero [of its *logical probability*] . . . for a general law . . . really counter intuitive?' But Popper never said that it was. He asserted, just as he did twenty years ago, that the logical probability of a universal law is in fact *zero*. At the same time, he also asserted that *the confirmation or corroboration* of a well tested law may approach the degree *one*.

hit by the paradox of confirmation (discussed in *Probability*, p. 469), as Dr Bar-Hillel will no doubt see at a glance if he compares the two pages.

Now Dr Bar-Hillel says that he has glanced but in vain; and he asserts that the qualified instance confirmation is not hit by the paradox of confirmation. But only a few lines later, he admits everything—in a sentence beginning with 'He [Popper] seems to have been misled'.

What Dr Bar-Hillel admits is the fact that the laws, l_1 and l_2 , even if they are *logically equivalent* (i.e. *merely linguistically or verbally different*) 'need not have the same qualified instance confirmation'.

But this fact constitutes precisely the so-called paradox of confirmation: an engineer building a bridge who asks himself what is the reliability of the law l on which he bases his calculations will be deeply disturbed if he is told that this depends entirely on the choice of words in formulating the law; for example, the law 'No raven is white or brown or yellow', has a much lower 'qualified confirmation' (in view of the rarity of ravens) than the logically equivalent law 'No thing that is white or brown or yellow is a raven'. If the engineer's knowledge of Aristotelian logic, or his logical intuition, tells him correctly that these two laws are only verbally different, then he will hardly be satisfied by Professor Carnap's assertion that his qualified instance confirmation 'represents still more accurately what is vaguely meant by the reliability of a law'.

How then can Dr Bar-Hillel deny that this 'explication' of the idea of reliability is hit by the paradox of confirmation? The answer seems to be that he has too much faith in symbols, and in the distinction between a word-language and a symbolic language: Professor Carnap speaks (on p. 572, line 3 from the bottom of the page) explicitly of the 'qualified instance confirmation of the law'— x , say—given the evidence y . But he does not introduce a symbol for it, such as

$$Q(x, y).$$

Instead, he introduces a symbol,

$$(S) \quad c_{ia}(M, M', y)$$

where ' M, M' ' together describe the particular linguistic form in which the law x has been formulated. The symbol (S) is then unambiguously defined, and so Dr Bar-Hillel thinks that the paradox does not arise. But this clearly is ostrich policy.

The paradox appears as soon as we interpret the symbol (S) in the word-language. And Professor Carnap interprets it to mean (using twice the definite article 'the') 'the qualified instance confirmation of the law'; say, of the law x (a law one of whose several logically equivalent linguistic formulations is indicated by ' M, M' '), given the evidence y . This is how the concept is explained; and this explanation amounts to a licence to introduce the symbol

$$(S') \quad Q(x, y)$$

for it; of course, the actual introduction, or actual non-introduction of this symbol by Professor Carnap cannot affect the question whether the concept is, or is not, an 'accurate' way of rendering the intuitive idea of the reliability of a law.

Professor Carnap discusses the concept of 'the qualified instance confirmation of the law l [given the evidence e]' and its adequacy at considerable length, but he mentions nowhere that intuitively it is absolutely inadequate—as inadequate as anything can be

ADEQUACY AND CONSISTENCY

that is hit by the paradox of confirmation. This is also admitted by Dr Bar-Hillel. 'It is true', he writes, 'that Carnap did not mention this point explicitly, but then he promised to deal more extensively with the whole topic in vol. II of *Probability*. . .'

This remark does not disclose whether or not the glaring inadequacy we are here discussing was known to Professor Carnap, or whether I discovered it. And if it is I who have found that the paradox of confirmation squarely hits this concept, then I hardly deserve to be blamed for pointing out the inadequacy of an inadequate idea. But not only does Dr Bar-Hillel here, and in the next paragraph but one, speak about *my* misunderstandings: he even explains (in the paragraph between these two, beginning with the words 'What has to be understood. . .') another point of the 'current theory' which I have pointed out (apparently for the first time), and which I have criticised. The point is that Professor Carnap nowhere warns his readers that his two concepts of instance confirmation (Dr Bar-Hillel only refers to the second) *are simply not confirmation-concepts*, in Professor Carnap's sense, i.e. they are not 'regular *c*-functions' (or even *c*-functions at all). This is now admitted by Dr Bar-Hillel—again, in form of an explanation of *my* misunderstandings, as shown by the first words of the next paragraph 'The same misunderstanding made Popper claim . . .'.

But in this paragraph which begins so aggressively Dr Bar-Hillel ends by admitting everything, and he repeats *my* explanations, pointing out again *my* discovery (if it is a discovery; see my first reply, p. 160 lines 10 to 5 from the bottom of the text) that the two 'instance confirmations' are not 'confirmations' at all.

Moreover, Professor Carnap's concept c_i was supposed to 'explicate' the intuitive idea of the reliability of a *law*, as needed by a working engineer. Now Professor Carnap's engineer will hardly think a 'law' very reliable if it allows his bridge, or his plane, to crash, on the average, at every thousandth instance of usage or perhaps at every thousandth observation. But the 'current theory' attributes to a 'law' of this kind a degree of reliability very close to 1 i.e. to the maximum degree.¹

There is another little *paradox of instance confirmation* (both unqualified and qualified). It is connected with additivity. Neither Wald nor I accept additivity as a criterion of adequacy (cf. my first reply, lines 26 to 29 on page 162), and Professor Carnap rejects Wald's theory largely for this reason (*Continuum*, p. 85). Thus it may be assumed that he treats instance confirmation as *additive*. But in this case, the factually *false* disjunction of the two laws 'All swans are white' and 'All swans are non-white' has the same instance-confirmation—i.e. the same degree of reliability—as the *logically true* law 'All swans are either white or non-white'.

(8) 'The seemingly strongest objection of Popper's', writes Dr Bar-Hillel, '. . . is based upon a simple confusion, as Carnap shows in his *Remarks*, and will therefore not be discussed here.'

Here is a novelty indeed: for once my point is not admitted. This fact forces me to reply at length to section (c) of Professor Carnap's 'Remarks', and to Dr Bar-Hillel's dismissal of my 'seemingly strongest objection'.

What I asserted (both in my 'Degree of Confirmation' and in my first reply to Dr Bar-Hillel was this.

¹ In view of Dr Bar-Hillel's first footnote, I must make it quite clear that I was merely *criticising* the two concepts of instance confirmation, and that my 'rectification'—which indeed works—was part of my criticism. I myself would not dream of accepting any of these concepts, or any similar one, whether 'rectified' or not.

Writing ' $p(x, \gamma)$ ' for any probability function, or more precisely, any function concerning which all parties agree that it satisfies the well known rules of the calculus of probabilities, I asserted that

(3p) If x follows from γ , then for every z ,

$$p(\gamma, z) \leq p(x, z),$$

since (3p) itself is one of the well known rules of this calculus. But I denied that the analogue of (3p) for *degree of confirmation* is true; i.e. I denied,

(3c) If x follows from γ , then, for every z ,

$$C(\gamma, z) \leq C(x, z).$$

I proved in 'Degree of Confirmation', page 144, that (3c) does not hold for any function $C(x, \gamma)$ which satisfies a simple adequacy condition (see (C) below) for degree of confirmation.

In view of section c of Professor Carnap's 'Remarks', I now offer a new argument against (3c): I assert that (4), which he rejects, is derivable from (3c). I write (4) as follows:

(4) If x follows from γ , then, for every z ,

$$\text{if } p(\gamma, z) > p(\gamma) \text{ then } p(x, z) > p(x).$$

Both Professor Carnap and I agree that (4) is logically false. We also agree that (3p) is true. Of 'its analogue for degree of confirmation', i.e. of (3c), Professor Carnap now again asserts that it is true, while I, on the contrary, assert that it must be false. I shall now establish its falsity by deducing (4) from it. For this purpose I only need the following true (because tautologous) conditional (t):

(t) If x is not confirmed by z , and γ is confirmed by z , then it cannot happen that x is as well confirmed by z as γ , or better confirmed by z than γ .

In order to express this symbolically, I introduce the symbol ' $\text{Co}(x, \gamma)$ ' by the symbolic convention:

(A) We write ' $\text{Co}(x, \gamma)$ ' for ' x is confirmed by γ '.

' $\text{Co}(x, \gamma)$ ' can, of course, also be read ' γ confirms x ', or ' γ (positively) supports x ', etc.

We can now express (t) symbolically by

(tc) If not $\text{Co}(x, z)$, and $\text{Co}(\gamma, z)$, then not $\text{Co}(x, z) \geq C(\gamma, z)$.

My assertion that (t) and its equivalent (tc) are tautologous, or analytic, is not merely based upon intuitive grounds. It is based upon the fact that every statement is true which results from substituting in (t) a grammatically fitting predicate for 'confirmed' (or 'confirmed by z '). For (t) is an instance of the general schema,

(t.o) If x does not possess the property P , and γ does possess the property P , then it cannot happen that x possesses the property P in the same degree as γ , or in a higher degree than γ .

This schema applies not only to properties (one-termed predicates) such as Professor Carnap's own examples of *Probability*, §§ 4, 5, and 8, where he discusses the predicate 'warm', but also to all grammatically fitting relational predicates (including even comparatives). This may be seen by trying out such physical

ADEQUACY AND CONSISTENCY

predicates as ' γ is lighter than z ', or ' γ is inflated by z ', or such psychological predicates as ' γ is more enlightened than z ', or ' γ is indoctrinated by z '; or ultimately such logical predicates as ' γ is made more probably by z than γ was before ', or ' γ is probable, given z '. In view of §§ 4, 5, and 8 of *Probability*, we may say that $(t.o)$ expresses a *necessary condition of adequacy*¹ for any pair of ('classificatory' and 'comparative'—or rather 'graded') predicates, such as the pair 'warm' and 'warm in a higher degree', in the sense that the definitions of any such pair must entail the corresponding instance of $(t.o)$.

Now from $(3c)$ and (tc) we obtain by propositional logic alone :

(3d) If x follows from γ , then, for every z ,
 if $\text{Co}(\gamma, z)$ then $\text{Co}(x, z)$.

This is Professor Carnap's 'special consequence condition', declared to be invalid on the bottom of page 474 of *Probability*. It is equivalent to (4), as he himself shows in section *c* of his 'Remarks', referring to *Probability*, page 464 (4) which amounts to

(B) $\text{Co}(x, \gamma)$ if and only if $p(x, \gamma) > p(x)$.

For by substituting in $(3d)$ in accordance with (B) we obtain,

(3d.1) If x follows from γ , then, for every z ,
 if $p(\gamma, z) > p(\gamma)$ then $p(x, z) > p(x)$,

which is Professor Carnap's (4). His system thus contains precisely that 'glaring inconsistency' which he so describes in section (c) of his 'Remarks'—unless he should take the desperate step of rejecting (t) *ad hoc*, thereby creating a new inconsistency which to the 'unguided' eye might be even more glaring.

In parenthesis it may be said that both (tc) and (B) are obvious consequences of the following condition (C) which is part of my first '*desideratum*' in 'Degree of Confirmation' (cf. pages 143f. and 147) :

(C) $C(x, \gamma) > 0$ if and only if $\text{Co}(x, \gamma)$.

Thus (C) allows us to derive (4) from $(3c)$. But I do not now employ (C) in this reply, because Professor Carnap refers me to page 475 of *Probability* where I find a warning against the use of an idea which seems to be the same as my ' $C(x, \gamma) > 0$ '. This idea is indicted for linking $(3c)$ with (4); but in fact (4) can be derived from $(3c)$ without it.

Thus $(3c)$ is again refuted, and with it the thesis that there exists an adequate confirmation concept satisfying the rules of the probability calculus.

This is as it should be. For $(3c)$ would prevent Maxwell's electromagnetic wave theory of light from ever becoming more highly confirmed than Fresnel's wave

¹ Dr J. Agassi has drawn my attention to the validity of a stronger adequacy condition

($t+$) If $\text{MC}(x, z, \gamma, w)$, then, if $\text{Co}(\gamma, w)$ then $(\text{Co}(x, z))$
 (where $\text{MC}(x, z, \gamma, w)$ if, and only if, $C(x, z) \geq C(\gamma, w)$, as in *Probability* § 80).
 Dr Agassi's condition can be generalised for any relation, of any degree, which is capable of gradation.

theory of light ; and this in spite of the fact—which entails the rejection of (3d)—that Maxwell's theory is supported by more and by stronger empirical evidence than Fresnel's. But this is just my point about *content* whose neglect has rendered either inconsistent or inadequate all those variations upon the theme 'degree of confirmation' which have cropped up during the last twenty years.

In view of the extreme triviality of my derivation of (4) from (3c) it is a little hard to understand how my point could still be contested with so much confidence. One reason may have been that (4) is demonstrably independent of (3p). This may have been misinterpreted—especially if (3p) and (3c) are confused—as a formal proof that (4) is independent of (3c) ; although an independence proof of this kind clearly becomes invalid the moment any new formulae—say, some of the adequacy conditions implicit in *Probability*—are conjoined to (3c). But a deeper reason may well have been this. The leading idea of *Probability*—that inductive logic is a generalisation of deductive logic—suggested that degree of confirmation is a generalisation of derivability and that it can therefore 'explicate' logical probability ; but this entails the dogma that degree of confirmation must satisfy the rules of the probability calculus, and thus (3p) which is one of them. (Incidentally both this leading idea and this dogma lead to many further results which are even worse.)

(9) Being myself a party in this dispute, rather than a judge, I feel that it would be slightly comical if I were to sum up by saying that Dr Bar-Hillel's strictures are 'objectively unfounded' ; but I repeat that he has silently admitted most of my 'objectivity unfounded' criticism ; just as he now silently admits the invalidity of his own *Dictionary*.

He ends up most generously by crediting me with finding weaknesses in the 'current terminology'. But I was not concerned with terminology ; my point was that there is no current *theory* of confirmation.

I hope that by now we have exhausted the topic raised by the ten-line footnote on page 144 of my note 'Degree of Confirmation'. May I suggest that there are other and perhaps more interesting passages in that note of mine ?

K. R. POPPER

REVIEWS

Facts and Faith: The Dual Nature of Reality. By REGINALD O. KAPP.

Riddell Memorial Lectures, twenty-seventh series. The University of Durham. Oxford University Press, 1955. Pp. 63. 5s.

As their title suggests, these lectures are partly concerned with the conflict between science and religion. But their main theme is better indicated by the sub-title 'The Dual Nature of Reality'. The author's aim is to present the case for a dualistic philosophy of nature and against a monistic one. This issue, he argues, is of enormous importance for the philosophy of science, but is less important for theology than is commonly thought. 'I doubt indeed whether monism is bad religion. But I am sure that it is bad science' (p. 14).

Possibly his use of the word 'monism' may cause discomfort to some of his readers. The monistic theories which are most familiar to students of philosophy are idealistic ones, such as the philosophy of Hegel or of Bradley. Professor Kapp is of course well aware of this. But he himself means by 'monism' *materialistic* monism, and he says so explicitly on page 9. The reader must not be misled by the word 'dualism' either. The dualism which Professor Kapp advocates is not the classical dualism of Descartes. He rejects Descartes's mechanistic biology. His own dualism, if we must assign a label to it, is a form of vitalism, in which conscious mind is only one among several kinds of 'diathetes' or non-material controlling agencies (*διαθέται* from the Greek verb *διατιθέναι* 'to arrange'). Thus the diathete which controls the growth and other vegetative processes of an oak tree is not a conscious mind. 'Dualism is the belief that the whole of reality is composed of two parts, one named matter and the other diathetes. Some events are held to happen because those two component parts act on each other' (p. 9).

Professor Kapp holds that a diathete is a non-spatial entity. It has no location and *a fortiori* no shape nor size. We have to say of it that it exists and yet is nowhere. He seems to think that this will be regarded as a serious objection to his thesis. I do not quite see why. There is no *a priori* reason why everything which exists must be somewhere. Moreover, it would seem that some of the entities spoken of in modern microphysics are not located in the plain unambiguous sense in which macroscopic objects such as brickbats are located. And Professor Kapp himself insists that even though a diathete is nowhere, what it *does* is somewhere. Thus Mr Jones's mind is a diathete, and the effects which it causes are located in Mr Jones's cerebral cortex, even though it itself has no location. Again, God may be regarded as the supreme diathete. Religious people are accustomed to say that God is everywhere ('omnipresent'). But as Professor Kapp points out, this can be taken to mean that He *acts* everywhere, that every event in physical space is subject to His control.

REVIEWS

How are we to decide between these two philosophies of Nature, materialistic monism on the one hand, and vitalistic dualism on the other? We might try to do so by considering the properties of mind. 'The reality of thought and feeling, of all subjective experience, is used as a proof that the objective world, as revealed by a study of matter, does not encompass the whole of reality' (p. 16.) Professor Kapp admits that there is some force in this argument, but thinks that it may lead to an incomplete picture of reality. It may lead us to suppose, with Descartes, that the only diathetes are *minds*. 'The distinction between conscious behaviour and vegetative processes is treated as basic, and the distinction between the organic and the inorganic world as superficial.' But this is a mistake. 'The true dividing line, . . . separates all living from all lifeless substance' (pp. 16-17).

Nor must we suppose that the action of diathetes is only detectable when we reflect upon specially valuable objects, such as Shakespeare's plays or Beethoven's symphonies; nor again that it is only detectable when we consider very unusual phenomena, such as extra-sensory perception. On the contrary, 'the most relevant facts . . . are the very familiar ones. They are so familiar to us all that we tend to overlook their significance' (p. 18). The crucial point is that some events display *order* and other events do not. If matter has no properties but those which are listed in textbooks of physics (and monists maintain that it has no others) monism has no means of explaining this empirically discoverable difference between the ordered and the non-ordered. We can only account for this difference by supposing that some physical events are affected by non-material agencies, diathetes, while other physical events are not. That is Professor Kapp's contention.

Evidently he must maintain that in what he calls 'the rough untouched world of lifeless things' there is no order. He does not, of course, have to hold that order is found only in living creatures. A building, a machine, the row of pebbles along the side of a garden path, a bird's nest, an anthill—a" these do display order in his sense of the term. But such material objects, though lifeless, are not 'rough and untouched'. The order they display is an effect of what living organisms have done to various pieces of non-living matter, and is therefore an indirect effect of the operation of diathetes. What Professor Kapp must maintain, and does in fact maintain, is that no order is discoverable in those parts of the material universe which are neither themselves living organisms nor affected by the activities of living organisms. He rightly anticipates that this very radical contention will shock some of his readers. Ever since the Psalmist exclaimed 'The heavens declare the glory of God' religious people have liked to think that the physical universe is ordered through and through. Tough-minded materialistic persons have liked to think so too, though from different motives. They have wanted a deterministic world. And so they have held, like their religious opponents, that there is what Professor Kapp calls 'a cosmic statute book', though they

have rejected the argument that if there are such cosmic statutes, there must be a Sovereign of the Universe who wrote them.

In Lecture II, 'Is there a cosmic statute book?', Professor Kapp argues that in actual fact there are no such statutes at all, or at any rate that the progress of physics, from Newton's time onwards, has reduced the cosmic statute book to a very slim volume indeed. Eddington has shown that the law of gravitation, the laws of mechanics, and the laws of the magnetic field can all be summed up in one principle, the Principle of Least Action, and that this could equally well be called the Principle of Greatest Probability. Professor Kapp quotes Eddington's statement 'The law of nature is that the actual state of the world is that which is statistically most probable' and makes the following comment on it: 'This is the state in which the traffic in our streets would be if there were no rule of the road; it is the state in which all things find themselves when everything is allowed and nothing prohibited' (pp. 31-32).

If the physicists tell us that this is what the physical world is like, no doubt the ordinary man will have to take their word for it. And yet he will be inclined to protest that in the *macroscopic* physical world (the only world which he can observe) there is plenty of order all the same. At any rate there are discoverable regularities in it. If not, how could induction generalisations be made and relied upon for anticipating the future, or more generally for inferring from the observed to what is not at present observed? Indeed, how could men, or animals either, manage to learn anything from experience at all? Physicists will no doubt point out to us that inductive generalisations about the macroscopic physical world can only tell us, at the best, that given the occurrence of *A* the occurrence of *B* is exceedingly likely, not that it is absolutely certain. But this will not worry the ordinary man at all. He will be quite content to believe that water nearly always flows downhill and almost invariably boils at 212° Fahrenheit at sea level. If there is good ground for such generalisations as these (and surely there is), he will say that there *is* order in macroscopic lifeless things. He does not ask for rigid determinism, but only for dependable and empirically discoverable regularities, and these he has. So far as I can see, the progress of physics has done nothing to show that these macroscopic regularities do not exist. What it has done is to *explain* this macroscopic order by showing that it can be inferred from statistical propositions about very large aggregates of microphysical events which are individually unpredictable. And this is a very remarkable achievement. But after all, one does not abolish empirical facts by giving an explanation of them.

When we reflect upon our experience of the macroscopic world, the distinction which first strikes our minds is, surely, not the one which Professor Kapp emphasises, between the orderly and the orderless or random but rather the difference between two contrasted types of order or regularity,

REVIEWS

the teleological type of order and the non-teleological type of order. It is indeed difficult to understand how a monistic theory can account for this difference, or how the teleological type of order could exist at all, given the two assumptions (1) that the universe is wholly material, (2) that matter has only those properties which are attributed to it in textbooks of physics. In these lectures, however, Professor Kapp does not have much to say about the concept of teleology. He sometimes gives the impression of holding that all order is *ipso facto* teleological; and this would seem to be a very questionable assumption. Perhaps the difficulty here is partly one of terminology, and some other word than 'order' might have been used for characterising that which is absent from the 'rough untouched world of lifeless things' and present in organisms and their products.

There are a number of other interesting points in these lectures which the present reviewer is not competent to discuss: for example, in the section on 'Order and the Second Law of Thermodynamics' in Lecture III (pp. 41-45) where it is suggested that this law provides 'an objective criterion of order' and a measure of the intensity of the 'diathesis' to which a given material system is subjected. But perhaps enough has been said to arouse the curiosity of the reader. Anyone who is interested in the philosophy of nature should read this little book. Professor Kapp has stated an unfashionable thesis in a very forcible and downright manner. His arguments for it are neither metaphysical nor theological, but scientific. He claims to show that materialistic monism is 'bad science'. If his contention is to be rejected, his arguments ought to be examined and answered.

H. H. PRICE

Kant's Metaphysics and Theory of Science. By Gottfried Martin. Translated from the German by P. G. Lucas.

Manchester University Press, 1955. Pp. viii + 218. 25s.

THIS book contains an interesting, vivid, and clearly written account of Kant's ontology and philosophy of science. Kant's position is explained by relating it to the positions of such philosophers as Plato, Aristotle, St Thomas, Ockham, but notably to the philosophy of Leibniz on the one side and to contemporary science on the other ('The real discussion of Leibniz' philosophy is, if we are right, the philosophy of Kant', p. 7). The author attempts to show 'the inner connection between ontology and theory of science' (p. v), which are in his opinion the two main streams from which the *Critique of Pure Reason* is fed (p. 16). Accordingly the book divides into two main parts, the first part being a 'running commentary' (p. 43) to the *Critique* which is here mainly considered as an essay in philosophy of science; part two discusses the ontological aspects of the three

Critiques and their relation to 'the three great themes of God, freedom, and immortality' (p. 129). Although this second part contains a highly interesting account of the *Ding an sich*, we shall in this review deal with the first part only.

Professor Martin starts with an account of the problem of space and time. The well known assertion that the propositions of geometry are synthetic *a priori* is interpreted to be 'Kant's way of formulating the view that geometry is axiomatic' (p. 18), i.e. that the propositions of geometry follow from 'genuine axioms' rather than from definitions as Leibniz had assumed. Applying this interpretation to arithmetic, Professor Martin tries to show that Kant had recognised the defects of Leibniz's attempt to derive arithmetic purely from definitions (a discovery which is usually attributed to Frege), i.e. that he had recognised that in proving, e.g. $7 + 5 = 12$ by putting successively

$$7 + 5 = 7 + (4 + 1) = 7 + (1 + 4) = (7 + 1) + 4 = 8 + 4 \dots$$

(($7 + 1 = 8$ and ($4 + 1 = 5$ being given by definition) a commutative and an associative law have to be applied, laws, which are neither definitions, nor derivable from definitions alone (p. 26). This being so, however, we should have to admit that even formal logic is synthetic in character because it contains such statements as $a \equiv a$ which are synthetic in Kant's terminology (not derivable from definitions via the law of excluded middle), a fact which has been recognised by Russell (*Principles*, p. 457) which is duly emphasised by Professor Martin (pp. 88, 208), but which was already known to Kant himself. The queer conclusion is that Kant drew attention to a (much neglected) feature of formal discourse which is common to logic, geometry, and arithmetic, without in the end being able to distinguish—on the basis of this feature—between logic and, say, Hertz's mechanics. This leads at once to Kant's assertion that arithmetic and geometry are *constructive*. For Kant *does* distinguish between 'reason out of concepts' and 'reason out of the construction of concepts'. In his pre-critical writings he does in fact admit the logical possibility of non-Euclidean geometries (being a friend of Lambert he knew very well that Euclidean geometry was not the only one possible), but he denies that their concepts can be said to *exist* in a mathematical sense. This attitude of Kant's is linked by the author with such modern ideas as constructivism (cf. p. 26: 'Mathematical thinking is [for Kant] not merely thinking with pure concepts, it is also constructing, . . . operating'). Intuition, then, is not 'an additional source of knowledge for mathematics . . . , but is the factor which limits the broader region of logical existence . . . to the narrower region of mathematical existence' (p. 25)—a distinction which at the present moment is again being blurred by the fact that even logic has been found to divide into parts which are legitimate in the eyes of a constructivist and into other parts which do not possess this desirable quality. Which, together with

the remark made above, shows that the singular position, within Kant's system of Euclidean geometry does not indicate any 'natural' singularity (as Professor Martin seems to imply : cf. pp. 25, 40). Of course the fact that Euclidean geometry was the first geometry to be studied had a remarkable influence on the expressions used within non-Euclidean geometries (this influence has in the meantime completely disappeared). But expressions are not concepts. Only if one is fascinated by words one can, as Professor Martin does, arrive at the conclusion that the problem of the relation between Euclidian and non-Euclidian geometry is still 'completely open' (p. 40). This remark leads at once to the similar question whether or not Newtonian theory 'is in respect of its fundamental laws the only possible physics' (p. 90). For here, too, Professor Martin is seduced into thinking that 'classical physics would, indeed, stand as an *a priori* to quantum-physics' and, as one may guess, to any physics to come. There are now many physicists who think along similar lines (N. Bohr, Weizsaecker in his numerous presentations of Bohr's views, David Bohm). The philosophical background is very clear too—it is the background of such philosophers as H. Dingler, E. May, G. Hermann, who have in their mistaken apriorism (which is, indeed, a concealed positivism) completely confused the issue.

Returning to Professor Martin : A vivid account of the antinomies introduces us into Kant's theory of nature. Due attention is paid to the fact that those antinomies 'force us to regard physical theories as models produced by men' (p. 63). But Professor Martin goes a bit further. For him there is a direct relation between Kant's problems and (a) the set-theoretical antinomies (p. 54) and (b) the duality of interpretations in classical physics and in quantum physics (p. 53). 'Newtonian mechanics' Professor Martin says in commenting on the latter point, 'can be presented in different forms' (p. 53), e.g. as a mechanics of mass-points as well as a mechanics of continua. 'This conflict', he continues, 'has not reached any decision and [producing a typically philosophical verdict] cannot reach any.' This is true if Newtonian *theory* is thought to be the only basis of decision, for this theory is 'invariant with respect to' the above-mentioned 'variations'. But it does not at all follow that 'we can choose freely between these hypotheses' as is shown by the fact that although within quantum mechanics we possess at least three different kinds of statistics relative to which the theoretical background of quantum mechanics proper is 'invariant' we are nevertheless able to decide in favour of the one or the other ; and classical statistical mechanics definitely favours the particle-aspect. True, this was not known in Kant's time. But according to Professor Martin such instances could *never* be known—and here he is mistaken.

Turning now to Newtonian physics proper, we find again that its principles are assumed to be synthetic, i.e. not provable on the basis of the

REVIEWS

law of contradiction alone. But whereas the axioms of geometry are such that the whole of geometrical propositions can be derived from them, the axioms of theoretical physics as presented by Professor Martin (pp. 76f.) do not seem to be of that kind: for the law of causality, the law of substance, etc., are by no means sufficient for deriving the only possible nature, viz. Newtonian nature (p. 97). The derivation of the former principles is duly criticised (pp. 79f., esp. p. 89). This criticism is impaired by the fact that the relation between logic proper and scientific theories is not clearly apprehended (cf. p. 88): For the growth of, say, physical theories might necessitate a revision of logic (and not only lead to establishing a new synthetic discipline). On the other hand, it does not go far enough because, as we said above, Newtonian physics cannot be derived from the principles¹ (a fact which Kant seems to have recognised in the later *Metaphysical First Grounds*). This makes it even more clear that, on the basis of the analysis offered by Kant, Newtonian physics cannot be claimed to be the only possible physics, and thus there is no need for systematic philosophers 'to pursue this approach further' as the author suggests (p. 90). We must admit, however, that the function of physical theories had been greatly clarified by Kant. This is impressively shown on pages 90-126, where Kant's concept of nature as well as the unifying function of scientific theories is discussed. Once more we experience 'the inexhaustible depth of Kantian thought' (v). And having led to this experience is one of the main virtues of the book.

P. K. FEYERABEND

Alfred North Whitehead: An Anthology selected by F. S. C. Northrop and
Mason W. Gross.
Cambridge University Press, 1953. Pp. 928. 75s.

DURING the last decade interest in Whitehead's work has been slight. This is partly due to the anti-metaphysical trend of modern philosophy and partly to the extreme difficulty of understanding his later work. It is sometimes overlooked, because of this, that he was the co-author of *Principia Mathematica*, and that the central doctrines of his later metaphysical writings have their origin in his preoccupation with mathematical logic and its applications. However, with the recent appearance in the United States of a number of books relating to Whitehead, of which this volume is one, there has been some resurgence of interest in his writings, and even positivist reviewers have noted their modern ring. The reason is not far to

¹ There are some philosophers who seem to think that the whole of quantum-theory is contained in the principle of complementarity. This is just another mistake of the kind mentioned in the text.

REVIEWS

seek. At a number of points Whitehead's philosophy touches upon problems which are being discussed nowadays, if not by philosophers, at least by cyberneticians, psychologists, and philosophers of science. He was indeed one of the first to apply symbolic logic to concrete problems.

What this volume demonstrates, containing, as it does, selections from a large range of Whitehead's writings, is that there is a greater uniformity in his thought than has usually been assumed. In view of this it may be worth while giving some outline of Whitehead's philosophical doctrines, bringing out the relationship between his earlier more scientific philosophy and his later metaphysics. Three periods may be discerned in Whitehead's work, (i) that in which he busied himself with mathematics and mathematical logic, (ii) the period of his nature philosophy, in which he seemed to be putting forward a *Logische Aufbau der Welt*, showing how mathematical and physical concepts could be derived from experienced events, and (iii) his metaphysical period which largely begins with his translation to Harvard. It is the last which has aroused the most criticism and led him to be neglected by the more tough-minded philosophers.

A cursory examination of *Process and Reality* is sufficient to show that it is one of the most difficult of philosophical works written in the English language. The reader is faced with a formidable catalogue of such unfamiliar expressions as the Primordial Nature of God, the realm of eternal objects, conceptual and physical prehensions, subjective aim and concrescence—a vocabulary which seems shot through with subjectivism and is calculated to send a shudder down the spine of the semantically sophisticated philosopher. The whole picture seems very alien to his earlier writings, yet a closer examination will show that, despite the formidable terminology, the ideas contained in his later work are much simpler than is usually assumed, since he is working out some of his earlier ideas on a larger philosophical canvas.

In his earlier philosophy, developed in the *Concept of Nature* and the *Principles of Natural Knowledge*, Whitehead was mainly concerned with accepting the results of modern physics: physical nature as distinct from the immediately perceived events was taken as a four-dimensional structure of events pervaded by electromagnetic characteristics. Whitehead was not therefore simply putting forward a phenomenalism; he believed that the world described by the physicist really did exist, though perhaps not quite in the neat form it appeared in the physicist's equations. It is here that the *Method of Extensive Abstraction* proved its value, since as a logical instrument it enabled him, starting from sensory events, to arrive at such precise geometrical concepts as volumes, straight lines, points, etc., conceived of in the form of ideal convergent series. His early philosophy of nature therefore contains a *Logische Aufbau*, but one giving an independent status to the world of physics. A feature of this work as seen, for

REVIEWS

example, in the *Principle of Relativity*, was that he put forward his own brand of relativity theory, in which, unlike Einstein, he did not take physical space-time as heteroloidal, since he regarded it as having a similar uniform structure as perceptual space-time. Whitehead argued that perceptual space-time had to be uniform to allow for judgments of congruence upon which measurement was based, and that in order that induction should be possible the relations within our perceptual field had to be identical with those outside it.

At a first glance the philosophical system elaborated in *Process and Reality* seems a radical departure from his early writings. But when we examine what Whitehead means by speculative philosophy and his account of philosophical method, we find that it bears a close resemblance to the axiomatic method used by modern logic. However, Whitehead makes little attempt to formalise his account, except for one section in which he discusses the relationship of extensive connection (defined in terms of overlapping, etc.) holding between abstract topological regions. He further conceives these general topological relationships as forming an abstract system of order underlying the universe, and defining an infinite range of possible space-time orders, the present universe being regarded as one such value.

Against this background Whitehead works out his philosophy. Two main principles are postulated, the general scheme of order underlying the universe which guarantees induction (or God) and the physical events related within this structure (or the World). Further, he contends that the world of physics described in terms of the transference of energy in the electromagnetic field, has analogous properties to the sensory and emotional elements immediately experienced by us, as he describes the physical quanta of energy as primitive throbs of emotional intensity. For Whitehead the world is made up of co-ordinated systems of events or societies, some of which give rise to complex wholes manifesting new properties—life and mind, for example, are considered to be Gestalt properties of such systems of events. And since in his view psychological systems have in some respects similar properties to physical ones, there can, he assumes, be a causal interaction between them. He therefore believes that the free-will-determinism disjunction does not arise.

In his early writings Whitehead tended to gloss over the precise nature of the mechanisms involved in our perception of the external world, which at first sight made it appear that he was putting forward a variety of phenomenalism. In his later work he is considerably more explicit, since he asserts that the experienced sensory qualities are obtained by a process of statistical averaging from the physiological and physical activities involved in perception. A process which seems to resemble that postulated by some cyberneticians in their attempt to explain pattern recognition. Whitehead

REVIEWS

further believes that in our ordinary everyday life we use our sensory data as symbols for the actual events in physical nature. We are enabled to do this, he argues, since (1) they both partake in the same general scheme of order, and (2) our sensory data are causally related to the physical events in the immediate past which have given rise to them. Whitehead also attempts to overcome Hume's objection that causality is not a perceptual quality by postulating a special kind of causal perception. This, he asserts, gives us direct awareness of the functioning of our sense-organs and of the surrounding efficacious physical world; an awareness which he claims is more fundamental than that given by the specific colours, sounds, etc., in our perceptual field, which only have such an indirect reference to the physical events.

Though it is obvious that logic and mathematics play an important part in Whitehead's system, the part played by physical concepts has usually been overlooked. Despite Whitehead's claim that his account of perception is closer to the perceptual facts than that of other philosophers, he seems nevertheless to use physical field theory, as for that matter do the Gestalt psychologists, as a model for his descriptions of experience. When Whitehead therefore asserts that he is putting forward a philosophy of organism, this should not be taken to mean that he bases physics on biology, but merely that he believes they both deal with systems having a historicity about them. He is pointing out that in the physical world as well as in human experience the way things develop is determined not only by their present situation but by their whole past history. It is this which gives his philosophy a Hegelian tinge. In a nutshell then, the two key notions of Whitehead's later philosophy are the postulational method of modern logic with its emphasis on complex relational systems, and the field theory of modern physics with its emphasis on the historicity of physical systems.

Looked at in this way Whitehead's account does not seem to be as outrageous nor as metaphysical as some philosophers have made it out to be, since what he seems to be doing is a sort of applied logic. Even if one refuses to regard this kind of activity as philosophy, nevertheless there are today similar kinds of enquiry, for example, cybernetics, which are just as speculative, except that their speculations are decently veiled in mathematical formulae rather than philosophical language.

Of considerable importance in this anthology is Whitehead's 1906 Royal Society paper 'On mathematical conceptions of the material world', which is reprinted in full. Whitehead here attempts to apply the axiomatic method to physical science. It is interesting to note that his account of a many termed relation and its field, the entities related by it, in this case physical lines of force, bears a strong similarity to his account in *Process and Reality* of a general system of order permeated by electromagnetic events. The change in Whitehead's views would not then seem to be as radical as

REVIEWS

has usually been supposed, since in some ways he might be said to be returning to his 1906 position. The Royal Society paper probably gives a much clearer insight into Whitehead's later philosophy than the many analogies that have been drawn between his work and such philosophers as Plato.

If I have any criticisms to make of Whitehead's later position it is that I am sceptical of any attempt, philosophical or otherwise, to regard the universe as a logical system. I am also somewhat unhappy about Whitehead's identification of physical field theory with the experienced passage of nature; the former seems to be more of an abstract mathematical construction than an experienced fact. But nevertheless, it needs to be pointed out that quite a number of physicists would disagree with the phenomenalist approach and accept the reality of physical entities, though not perhaps Whitehead's account in terms of emotional energy.

Less than half of this anthology is made up of selections from Whitehead's earlier mathematical and philosophy of nature writings. My chief criticism is the method of selection adopted. I would have liked to have seen more of Whitehead's mathematical and philosophy of nature writings included. However, the editors have performed a most useful task in giving a representative sample of the more philosophical of Whitehead's writings. What is still required is an anthology dealing more specifically with Whitehead's contributions to mathematical logic and the philosophy of science.

W. MAYS

La logique et la science (Étude épistémologique). By P. Césari.
Dunod, Paris, 1955. Pp. 171

THIS little book is mainly a criticism of the formalist approach to logic and mathematics and the application of the axiomatic method in science. The science discussed is largely theoretical physics, and though there is some reference to biology, there is little or none to the human sciences. In many ways it follows the traditional approach of French thinkers who emphasise the intuitive or epistemic aspects of mathematical and scientific reasoning.

Césari rejects the view held by some philosophers, that in order to criticise scientific knowledge a general enquiry has first to be made into the nature of knowledge itself, and that this analysis is then to be imposed normatively on science. An epistemology which does not base itself upon some specific scientific subject matter is regarded by him as valueless, since he conceives its task as the examination of the hypotheses, conclusions, and methods of that science. Obviously, he has in mind here a methodology of science rather than a theory of knowledge as we know it in this country.

REVIEWS

Césari discusses the work of Comte whom he thinks to be the precursor of scientific epistemology, and compares Aristotelian logic with the algebra of logic. He also touches upon the inapplicability of the principle of excluded middle in micro-physics, the attempt to apply three-valued logics in this field, and the nature of axiomatic systems.

There has, Césari argues, been a neglect of the operational-epistemic aspects of logic and mathematics and an over-emphasis on their formalisation. The attempt to equate logical necessity with deducibility and non-contradiction is dismissed by him as an ideal doomed to failure. Though agreeing that many analytical operations occur in deductive mathematical processes, he thinks they have an essentially irreducible synthetic aspect. This he relates, as Poincaré does, to the principle of mathematical induction which enables us to extend non-contradiction indefinitely, and which lies at the root of the logico-mathematical paradoxes. He believes his position strengthened by Gödel's proof of the impossibility of demonstrating the non-contradictory character of mathematics.

Mathematical and physical reasoning have this in common for him; they are analogical in character and are concerned with epistemic processes. Césari is careful, however, not to identify these two fields of enquiry, especially as he rejects the *a priori* in science. Though physics is becoming more and more describable in terms of mathematical relations, this, he argues, is due to physical reality having a relational character.

One sympathises with the author's criticisms of the philosophical attempt to construct theories of knowledge *in vacuo*, and also to a more limited extent with his emphasis on the heuristic conditions of inference. Nevertheless, despite Césari's emphasis on the limitations inherent in the formalist programme, one can still go a long way with it. In some parts of logic and mathematics, decision problems are solvable and completeness demonstrable.

W. MAYS

Man on his Nature. By Sir Charles Sherrington.

Penguin Books, Harmondsworth, Middlesex, 1955. Pp. 312. 2s. 6d.

THIS book is too well known to require detailed notice, fifteen years after its first appearance, now metamorphosed into a 'Pelican'. Some general comments may be made. It is remarkable how unenlightening can be the reflections of an eminent physiologist on philosophical problems arising out of his researches. Nothing that is said about teleological explanation in Biology or the relation between Physiology and Psychology seems at all fresh or suggestive. The interest of the book lies in its long descriptive passages, some of which are highly metaphorical and rhapsodical. There is very little attempt to analyse biological concepts.

REVIEWS

It might be inferred from this that the facts and theories of science are irrelevant to philosophical questions. But clearly it would be futile to launch out into an examination of the concepts of space and time without heeding the theories of Physics. Why should it be thought that work on the concepts of body and mind can go on in happy independence of theories of nervous functioning? Such assumptions may mask a stubborn and complacent ignorance.

The proper inference surely is that scientific expertness is necessary but not sufficient. Lack of logical sophistication may impede the fruitful development of a problem. For example, Sherrington tries to pair off directly concepts of different logical depth, such as feelings of anxiety and changing electric potentials; when these 'remain refractorily apart', he falls into despair. Energy and mind are said to be 'phenomena of two categories', and it is implied that all statements within the 'energy-scheme' are directly testable by observation. No hint is dropped of the logical complexity of physical theory.

Conceptual co-existence may be the best that can be hoped for in the present phase of Psychology and Physiology, and Sherrington, as against the 'two-languagers', was surely right in being worried about this. However, the breach is unlikely to be filled without clearer ideas about the kind of unification that it is appropriate to seek.

R. J. SPILSBURY

Science et technique. By René Boirel.

Editions du Griffon, Neuchatel, Suisse, 1955. Pp. 116. Fr. 6.80.

THIS little book appears as one of a series, to which Lemaître, Pauli, and others have also contributed, on problems of the philosophy of science. This volume describes, with the aid of some historical examples, the inter-relationship of science and technique, showing how each is conditioned by, and necessary to, the other. Both are activities of the human spirit, but, at the same time, both result from the 'debate of bodily activity with matter', and the theoretical structures of science and the complex mechanisms of technique can alike be seen as extensions of primitive and childlike efforts to manipulate matter.

One cannot have everything in such a small space, and although the other aspect of science, that of seeking explanations, is neglected here, it is good to be reminded that scientific explanations themselves are often directly indebted to contemporary techniques, as in the case of Carnot and the steam engine, or Torricelli and the water pump. M Boirel might have added the influence of irrigation engineering and techniques of centrifuging upon the physiological explanations of the Hippocratic school.

MARY B. HESSE

REVIEWS

Déterminisme et indéterminisme. By Paulette Février.

Presses Universitaires de France, Paris, 1955. Pp. xii + 250. 1000 fr.

THIS book is based on an essay which was awarded the Prix Santour by the Académie des Sciences Morales et Politiques. It is a critical discussion of the concepts of determinism and indeterminism as they have developed in physics from its classical stage to the present day. There is no attempt to treat these questions as a 'general' philosophical problem. Madame Février curtly and sensibly dismisses in her first chapter the notion that there is any such single problem for philosophers which can be stated clearly enough to afford a useful basis for discussion. Nor does she consider the application of these concepts to any other subject-matter but that of physics.

To avoid any suppositions that are not experimentally testable, Madame Février defines determinism by means of predictability. She makes it clear that the adjectives 'determinist' and 'indeterminist' are applicable in her discussion to particular physical theories. Such a theory may be determinist (or indeterminist) *in fact* or merely *in principle*. Roughly speaking, a theory is determinist in fact if it enables us to make measurements from which we can deduce, in the context of the theory, reliable predictions. If a theory would enable us to make such predictions if we could make, as we cannot, appropriate measurements to determine the evidence for our predictions, it is determinist only in principle. Conversely, a theory indeterminist in principle shows us that certain predictions are impossible whatever the procedures of measurement at our disposal (that is to say, the impossibility of the predictions is a consequence of the theory). Finally, a theory is indeterminist in fact if, whatever the logical structure of the theory, we cannot make the measurements necessary as premisses for the prediction. There is thus an overlap between those theories which are indeterminist in fact and those which are determinist in principle. Theories in the first class may turn out to belong to the second class or, developed further, turn out to be indeterminist in principle.

Applying these notions to a survey of the history of physics during the past 150 years, Madame Février shows how the concept of determinism in this field has altered with the changes in observational techniques and with the adoption of the new mathematical methods needed to formulate new theories. Her main conclusion is that determinist theories are possible only where the 'facts' which the physicist deals with owe nothing to his position as an observer or to his theoretical assumptions. Although this conclusion is not new, the detailed evidence which is adduced for it and the careful account of the logical nature of the theories examined (which occupies the main part of the book) will be of great value and interest to the philosophically minded physicist.

D. J. O'CONNOR

ABSTRACTS

Dialectica, 1955, 9, Nos. 1/2

F. Gonseth, 'L'ouverture à l'expérience et les *a priori*'.

This article asks whether modern science rejects categorically the Kantian conception of the *a priori*. There is a real issue here and the author asks what authority would be relevant to and could decide it.

The problem of deciding it leads to a series of requirements: we require an open character for science as well as for philosophy. This condition, and those of relevance complementary to it, are not satisfied, except within an open theory of knowledge: generally speaking, we need an open philosophy by which the *a priori* concepts must be re-interpreted.

P. Bernays, 'Zur Frage der Auknüpung an die Kantische Erkenntnistheorie'.

The abstract will appear when the second part of this paper has been published.

H. Scholz, 'Eine Topologie der Zeit in Kantischen Sinne'.

This paper is concerned with the fundamentals of a topology of time, and discusses the temporal order as experienced, and the grounds of objectification. It goes on to discuss Kant's criterion for '*x* is earlier than *y*', and the creative and *a priori* character of the norms of objectified experience. It passes then to modern criteria of time based upon causation, the two methods of light-signals and irreversible processes, and their combination, and ends with a discussion of Russell's theory of temporal order.

P. Lachièze-Rey, 'Le kantisme et la science'.

Kant has contributed two basic ideas to the philosophy of science: Kantianism has shown how objective knowledge is constituted by the mind, according to certain rules; and it has shown up thereby the incompleteness of any theory which lets us manipulate objects without considering the problem of how the object is constituted. Thus Kantianism enables us to make clear the ontological status of its constructions and operations.

This philosophy develops (mainly through the theory of schematism) the idea of a transcendental subjectivity which includes the mobility of a body. In this way, the importance which modern philosophy attaches to the body itself and to intentional mobility is emphasised. This is needed both for the construction of the geometrical object and for the manner in which time itself is developed.

F. Gonseth, 'Connaître par la science (suite)'.

The first part of this article appeared in No. 31. The author avoids assuming any untestable hypotheses about the origins of man, the genesis of language, and the beginnings of scientific knowledge. Man always appears to us as a social being: he possesses a language and is capable of modifying his environment. The exact moment at which man with pre-scientific knowledge reaches scientific knowledge can hardly be determined.

The author establishes that the development of scientific knowledge goes parallel to the progress of scientific methodology and to the history of philosophy.

ABSTRACTS

In the development of science, three important periods can be distinguished : (i) rationality ; (ii) being open to experience and to experimentation ; and (iii) reflection. Here the theory of knowledge becomes a condition for determining the very means of knowledge.

We can equally distinguish three main aspects in the development of philosophy. But philosophy has not succeeded in linking the empirical with the theoretical factor satisfactorily. In order to remain on the level of our new scientific knowledge, philosophy must take the form of an open philosophy—open, both to the evolution of knowledge and to its own experience.

F. Gonseth, ' *Recherches méthodologiques* '.

This long article reproduces the conclusions of the author's work on ' Geometry and the problem of space '. The first volume of this work appeared in 1945, the sixth and last is just being published.

The author keeps it constantly in mind that the reconstruction of geometry from its most elementary to its most abstract aspects must be conducted as a methodological experience. This experience must guarantee the methodology of geometry, a methodology of the sciences, and a philosophy of knowledge.

The author returns to the different ways of achieving this and gives his results.

He shows how the discipline of geometry may be placed in a perspective that considers all its aspects. This leads to a methodology which may be integrated into a philosophy of open knowledge.

Dialectica, 1955, 9, 3/4

P. Bernays, ' Zur Frage der Anknüpfung an die Kantische Erkenntnistheorie '.

This discussion aims at extracting fruitful ideas from Kant for a theory of knowledge. (1) Kant's doctrine that we build up our empirical view of the world with the aid of intuition and thought has to be maintained ; but that laws of nature are due to the form of consciousness is denied. Thus we preserve his view that objects of experience are not prior to experience but constituted in the process of experiencing. (2) Kant's transcendental idealism is retained but without his basis for it. This gives science greater flexibility and realism. (3) Care must be taken about the way we distinguish our faculties of knowledge. (4) In modifying Kant, we are not offering a finished theory ; many questions must remain open.

F. Gonseth, ' *Des mathématiques à la philosophie* '.

Is there a methodological procedure for interpreting results from mathematics, say, into a philosophy ? Such could exist only if the philosophy contained the idea of *precision*. Neither Cartesian philosophy nor logical empiricism satisfy this test. But philosophy conceived as ' open ' does take account of the conception of the mathematics it uses.

F. Moch, ' *Oui, non—peut-être* '.

A working mind distinguishes three classes of judgments, inasmuch as it agrees, rejects, or reserves. Therefore its logic—the so-called Logic of Composition—uses

ABSTRACTS

three values ; this process reacts upon various activities. Its use makes it possible to define the 'logical instants' of any theory with precision. A chosen logic determines how general the corresponding notion of sets may be ; the logic of composition allows the highest degree of generality, which a two-valued logic could not reach ; and it reveals the precise relation between logic and set theory. It expresses mental structures necessary for any physics of probability, and, furthermore, for any undeterminist theory—in physics or in the theory of knowledge, as well as in ethics.

L. Rougier, 'L'Univers est-il rationnel ?'

The long debate on spirit and nature amounts to more and more evidence for the flexibility of the notion of reason. The stage of mechanical explanation led Jeans to hold that the world is more like a great thought than a machine, because it can be described mathematically. (In fact, modern physics is no more mathematical than formerly—it is simply more general.) But Jeans confuses pure and applied mathematics. Eddington claims to have deduced laws and physical constants from epistemological considerations. (This is a return to Kant.) But his theory provides neither for neutrons nor mesons. The possibility of finding a mathematical formalism applicable to experience has no bearing on the rationality of the universe, for a mathematician can always clothe an 'erratic' world in mathematical garb.

D. van Dantzig, 'Is 10^{10} a Finite Number ?'

Modern physics implies a limit to the number that can be constructed by the methods of the finitists, which is far below the number mentioned in the title. That number is therefore not constructable, unless the concept of constructibility is weakened to include steps that can be imagined.

This result seems contradictory, because it implies that a natural number cannot be constructed. But this pattern only gives the meaning of a changed use of the term 'natural number'.

The difference between finite and transfinite numbers cannot be defined operationally. The difference is not an essential but a gradual one. It becomes clear that intuition in mathematics cannot be maintained to be exact.

J. K. Feibleman, 'Knowing about Semipalatinsk'.

In the introduction to his *Human Knowledge*, Bertrand Russell wrote, 'If I believe that there is such a place as Semipalatinsk, I believe it because of things that have happened to me ; and unless certain substantial principles of inference are accepted, I shall have to admit that all these things might have happened to me without there being any such place.' Beginning with an examination of belief, the argument turns on the nature of evidence, and it is shown that Russell is in a contradictory position with respect to substance which he both rejects and assumes. A new definition of substance, one which will include a dynamic phase and also the objective existence of the irrational, is called for and offered. The new viewpoint involves us finally in the search for adequate descriptions, and here some notions of Wittgenstein and Russell seem to meet.

O. Bird, 'Dialectic in Philosophical Inquiry'.

Dialectic, as one of the methods of philosophy, is characterised by being propositional, interrogative, controversial, and interminable. These notes suffice to dis-

ABSTRACTS

tinguish dialectry from science, sophistry, rhetoric, and, generally, from intuitive and demonstrative knowledge. Each note also determines a function which dialectic fulfils in philosophical inquiry. The interrogative function is best seen in the consideration of principles and the formulation of problems. As propositional and controversial, philosophy depends upon the history of philosophy and is engaged in a continuous controversy which, as interminable, is capable of progress as dialectic ensures and promotes a community of understanding.

K. Miescher, 'Gegensätzlichkeit und Wirklichkeit'.

In an attempt to outline a system of reality, the author adopts the conception of a stratified structure and the 'categorical' mode of perception due to Nicolai Hartmann. He stresses the interplay of opposites. Total reality is comprehensible only if viewed from a dual aspect. The discursive-luminous aspect (spatio-temporal) is opposed to the intuitive-numinous one (not spatio-temporal); but they are complementary and both are needed to comprehend reality as a whole. A new development of older ideas, however, lead to an adequate systemisation of the sciences.

H. Dingler, 'Geometrie und Wirklichkeit'.

This posthumous article of H. Dingler reviews his operationalist theory of the applicability of geometry to processes of reality and the connection between technical operations and measurements.

J. B. Grize, 'L'implication et la négation vues au travers des méthodes de Gentzen et de Fitch'.

(1) The implication 'if . . . then . . .' has so essential a place in common thought that it is of the greatest importance to formalise it. But the classical formalisation presents some difficulties.

(2) The Gentzen L-method shows that common implication is better given by the positive part of the intuitionistic logic than by the classical one.

(3) Implication is linked to negation. Three concepts can be distinguished: refutability, absurdity, and falsity.

(4) The analysis of implication and negation is also possible with the Gentzen N-method. This procedure leads to a certain symmetry in the results.

(5) The F. B. Fitch logical S-system leads to a new kind of negation.

(6) The negation can be interpreted as 'contrary to facts'.

RECENT PUBLICATIONS ON THE PHILOSOPHY OF SCIENCE

(a) BOOKS RECEIVED FOR REVIEW

- G. C. Field, *Political Theory*, Methuen, London, 1956, pp. xvii + 297, 18s.
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- C. C. L. Gregory and Anita Kohsen, *A New Theoretical Basis for PSI*, Institute for the Study of Mental Images, Hants, 1956, pp. 36, 1s.
- Roy R. Grinker (Ed.), *Toward a Unified Theory of Human Behavior*, Basic Books, New York, 1956, pp. ix + 375, \$6.50.
- Heinrich Hertz, *The Principles of Mechanics Presented in a New Form*, Dover Publications, New York, 1956, pp. 271, \$3.50.
- Norman Howarth Hignett, *Portrait in Grey*, Frederick Muller, London, 1956, pp. 271, 18s.
- Rudolf Jordan, *Bridges to the Unknown*, The Saint Catherine Press, London, 1956, pp. 109, 10s. 6d.
- André Lamouche, *La théorie harmonique*, Vol. II, Gauthier-Villars, Paris, 1956, pp. 575, 1,400 fr.
- Bengt Lindegård, *Body Build, Body-Function, and Personality*, C. W. K. Gleerup, Lund, 1956, pp. 111.
- Mario Lins, *Logico-semantic Forms of Philosophical Inquiry*, reprinted from *Archivo di filosofia*, 1955, No. 3, pp. 35.
- E. L. Mascall, *Christian Theology and Natural Science*, Longmans, Green & Co., London, 1956, pp. xxi + 328, 25s.
- Herman Meyer, *Le rôle médiateur de la logique*, van Gorcum, Paris, 1956, pp. 239.
- John von Neumann, *Mathematical Foundations of Quantum Mechanics*, Princeton University Press; London, Cumberlege, 1955, pp. xii + 445, 48s.
- Otto Neurath, Rudolf Carnap & Charles W. Morris (Eds.), *International Encyclopedia of Unified Science*, Vol. I, Part 1, University of Chicago Press, 1956, pp. 339, £2 1s. 6d.
- Otto Neurath, Rudolf Carnap & Charles W. Morris (Eds.), *International Encyclopedia of Unified Science*, Vol. I, Part 2, University of Chicago Press, 1956, pp. 343-760, £2 1s. 6d.
- E. Tranekjaer Rasmussen, *Bevidsthedsliv og Erkendelse*, Munksgaard, København, 1956, pp. 199, Dan. kr. 12.50.
- Herbert L. Searles, *Logic and Scientific Methods*, The Ronald Press, New York, 1956, pp. viii + 378, \$4.25.

RECENT PUBLICATIONS

- Charles Smith, *Sensism: The Philosophy of the West*, Vol. I, The Truth Seeker Company, New York, 1956, pp. lvi + 732, \$10.00.
- Charles Smith, *Sensism: The Philosophy of the West*, Vol. II, The Truth Seeker Company, New York, 1956, pp. xviii + 1612, \$10.00.
- Alfred Tarski, *Logic, Semantics, Metamathematics* (translated by J. H. Woodger), The Clarendon Press, Oxford, 1956, pp. xiv + 471, 60s.
- Morton White, *Toward Reunion in Philosophy*, Harvard University Press, Cambridge (Mass.), 1956, pp. xii + 308, 45s.
- J. H. Woodger, *Physics, Psychology and Medicine*, Cambridge University Press, 1956, pp. x + 145, 8s. 6d.

(b) ARTICLES

- Louis de Broglie, 'El Problema de la Interpretación Causal y Objetiva de la Física Cuántica', *Suplementos del Seminario de Problemas Científicos y Filosóficos*, 1956, 4, 1.
- Jacques Hadamard, Alexandr D. Alexandrov, 'Las Definiciones Aziomáticas en las Matemáticas', *Suplementos del Seminario de Problemas Científicos y Filosóficos*, 1956, 6, 1.
- E. H. Hutten, 'Religion and the Physicists', *Rationalist Annual*, 1955, 47.
- E. H. Hutten, 'The Mistake of Modern Philosophy', *Literary Guide*, October 1955, 20.
- Paul E. Meehl and Michael Scriven, 'Compatibility of Science and ESP', *Science*, 1956, 123, 14.

MEETINGS OF THE PHILOSOPHY OF SCIENCE GROUP

The following meetings were held, by kind permission, in the Joint Staff Common Room, University College, London, during 1955-6:

1955

- 10 October: Address by the Chairman, Dr G. J. Whitrow, 'The Study of the Philosophy of Science'
- 14 November: Dr H. Ezriel, 'The Use of Psycho-analytic Sessions as Experimental Situations'
- 12 December: Dr A. W. Phillips, 'Control Systems in Economics'

1956

- 16 January: Mr Brian Ellis, 'Process and Non-process Explanations in the Physical Sciences'
- 13 February: Lord Halsbury, 'The Semantics of Determinism'
- 5 March: Annual General Meeting; Professor L. Rosenfeld, F.R.S., 'Success and Failure in the Growth of Science'
- 16 April: Mr G. Spencer Brown, 'Paradoxes of Probability'
- 14 May: Dr A. G. N. Weddell, 'Problems of Cutaneous Sensation'
- 18 June: Dr A. W. Stonier, 'The Influence of Philosophy of Economics'